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THE PSYCHOLOGICAL REVIEW

A PROPOSED REORIENTATION IN THE HEREDITY–ENVIRONMENT CONTROVERSY

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As more extensive and effective research procedures are brought to bear upon what has traditionally been termed the 'heredity-environment problem,' it becomes increasingly difficult for the psychologist to evade the issue on the grounds of inadequate data. The frequently repeated assertion that the crucial heredity-environment experiment has yet to be done is undoubtedly true, a fact which results in large part from difficulties in the control of experimental variables. But one wonders to what extent vague and unwieldy concepts may not have hampered the designing of definitive experiments in this area. We may be approaching a stage at which superficial methodological refinements and the accumulation of data are outstripping conceptual clarification.

When the psychologist is asked to define heredity, especially as it applies to the domain of behavior phenomena, the reply is frequently indirect, vague, or inconsistent. Nor are biologists always clear or consistent in their definitions. Thus McClung, for example, after a survey of the definitions of heredity given by various biologists, concluded that heredity has been variously conceived as a 'relation, act, fact, process, property, material, organization, rule, resemblance, or link.' [1] It would not be difficult to add to this list on the basis of psychological writings. The concept of heredity as a 'contributing influence' and as 'potentiality' would be two obvious additions.

The diversity of views presented by psychological writers in the area of heredity and environment may be analyzed in terms of a number of 'dimensions,' or specific respects in which they differ. These differences will be considered under the rubrics of: (1) the heredity-environment relationship, (2) the nature of heredity, and (3) the nature of environment. Against this background, the traditional 'heredity-environment problem' will be re-analyzed from a logical and operational viewpoint.

[1] *Cf.* McClung (12, p. 40), as well as the citation and fuller discussion of this point by Chein (4).

THE NATURE OF THE HEREDITY-ENVIRONMENT RELATIONSHIP

The interrelationship of hereditary and environmental influences in the determination of behavior has been envisaged in at least three major ways by different writers.

Isolated operation. The early classification of behavior into 'instincts' and 'habits,' corresponding to 'native behavior' and 'acquired behavior,' respectively, assumed the isolated operation of heredity and environment. Such a theory, implying the hereditary transmission of certain behavior functions *in toto,* has been quite generally superseded in contemporary psychology.[2] Although now generally admitted to be untenable, however, this belief that psychological traits can be separated into those which are acquired and those which are inherited is implied in a number of loosely expressed generalizations about the inheritance of behavior characteristics. Discussions regarding the inheritance of special talents or of such behavior patterns as 'hoarding' or 'collecting,' for example, frequently leave one with the impression that the inheritance of particular behavior traits as such was implied. Nor are more recent and more sophisticated psychological writings entirely free of such implications.

Independent additive contribution. A second possible way of conceiving the relationship between heredity and environment is in terms of a joint but independent contribution of an additive nature. According to this view, both heredity and environment contribute to all behavior development, the resulting behavior characteristics being analyzable into the sum of hereditary and environmental influences. That heredity and environment contribute jointly to behavior development is perhaps the most widely held of all views, but the additive assumption has been rarely made explicit. It should be noted, however, that this very assumption underlies all the recent attempts to determine the 'proportional contribution' of heredity and environment to the development of various behavior characteristics. The recent literature offers many estimated percentages purporting to show the degree to which intelligence test scores or some other index of performance depend upon heredity and upon environment.[3]

Interaction. The most widely recognized modus operandi of heredity and environment is that of interaction. According to this view, hereditary and environmental influences, however conceived, are regarded as mutually interacting factors in all behavior, the nature and extent of the influence of each type of factor depending upon the contribution of the other. Loevinger (11) has recently demonstrated the inconsistency of interaction with the additive assumption which underlies attempts to determine the proportional contribution of heredity and environment. Similarly, a number of years ago, Schwesinger (13) argued that the relative contribution of heredity and environment is specific not only to the trait, but also to the individual and the particular environment, *i.e.,* under different conditions of environment, the relative contribution of heredity will differ; and under different conditions of heredity, the relative contribution of environment will differ. Haldane (6) illustrates this point very dramatically when he writes that the principal cause of illiteracy among adults under forty in modern England is either mental defect or blindness; while in modern India, lack of educa-

[2] A comprehensive exposition of the arguments against this early 'instinct' view can be found in an article by Carmichael, appearing in 1925 (3).

[3] *Cf.* especially Loevinger's detailed analysis of the 'proportional contribution' studies (11).

tional opportunity is the principal cause. Thus in the former situation, educational history accounts for a very small percentage of the variance in literacy; in the latter, it accounts for a very large percentage.

It is clear that any estimate of the relative contribution of hereditary and environmental factors to individual differences depends upon the *range* or extent of both hereditary and environmental differences within the population under consideration. But this is by no means the only sense in which the rôles of heredity and environment are mutually interdependent. The nature and extent of the influence exerted by each type of factor depend upon the contribution of the other. In other words, any one hereditary factor would operate differently under different environmental conditions. Conversely, any environmental factor would exert a different influence depending upon the specific hereditary material upon which it operates. This is essentially what is implied by Woodworth's statement (17) that the same 'objective environment' may represent different 'effective environments' for individuals of varying heredity. Varying conditions of heredity lead one individual to 'select' in a given environment different influences from those 'selected' by another individual, and in different degrees. An obvious example is the effect of a radio program upon a congenitally deaf and upon a hearing child.[4]

[4] Of course, each individual's previous environment, as manifested through his reactional biography, likewise determines the 'selection' of influences in the present environment. This is the point made by Kantor (10, Ch. I, II, III) when he states that objects acquire specific 'stimulus functions' through the organism's contacts with them. Thus the individual's reactional biography determines the stimulus function of any part of the environment, *i.e.*, whether or not the object will serve as a stimulus at all and the kind of reaction which it will call forth.

Let us consider an hypothetical illustration involving intelligence test scores. Suppose we find a 10-point difference in IQ between two identical twins reared in separate foster homes, and a 30-point difference in IQ between two unrelated children reared in the same two foster homes as the twins. Can we argue that the 10-point difference between the identical twins measures the 'differentiating effect' of these two home environments, and can we therefore analyze the 30-point difference between the unrelated children into 10 points attributable to environment and 20 points attributable to heredity? Could we conclude that, insofar as these cases show, heredity was twice as important as environment in the production of individual differences in IQ? If we follow the concept of interaction, the answer to both questions is 'No.' Actually, a very slight hereditary difference between the two unrelated children may have greatly augmented the difference between the effective environments of the two foster homes, *i.e.*, between the active stimulus-value of the environments of the two unrelated children. The effective environmental difference between the two homes would thus have been much greater for the unrelated children than for the identical twins. No simple subtraction of the end-products could disentangle the relative contribution of the factors whose initial interaction led to the obtained difference in IQ.

It should also be noted that both heredity and environment represent complex manifolds of many specific influences whose relative weights may vary widely. In the previously cited illustration regarding the causes of illiteracy, for example, 'educational history' is only one specific aspect of environment, and could hardly be regarded as synonymous with it. In the same illustration, 'mental defect' and 'blindness' may themselves be the result of a wide variety of factors, environmental as well as hereditary. In the development of the individual, interaction occurs within as well as between the specific factors in each of the two categories. To speak of all the thousands of genes, each with its specific chemical and other properties, as though they represented a single force, operating as a unit to stimulate development in a particular direction, is highly misleading. It is even more clearly apparent that 'environment' is not an entity which can be contrasted or juxtaposed with 'heredity.'

Despite the almost universal acceptance of the interaction view of heredity and environment in contemporary psychological writings, statistical estimates of the degree to which 'heredity' and 'environment' account for the variance in one or another psychological trait continue to appear. It is interesting to note that some of the writers who have clearly argued for 'interaction' have themselves contributed some of these estimates of proportional contribution, presumably oblivious to the inconsistency of such a practice.[5]

Two other approaches to the analysis of hereditary and environmental contributions, although basically inconsistent with the interaction view, have found wide acceptance in recent years among psychologists who hold this view. One such approach is based on the assumption that we can measure *the* influence of heredity by keeping environment constant; and conversely, that we can measure *the* influence of environment by keeping heredity constant. It is doubtful whether the psychologically effective environment of two individuals can ever be kept constant. The condition of constant heredity, however, is fulfilled by identical twins, who have been eagerly sought by investigators for this purpose. Since the logic of the analysis is similar whether heredity or environment is kept constant, we may consider the comparison of identical twins as an illustration. Any observed behavior difference between such twins can undoubtedly be attributed to environment. The degree or extent of difference found in such a case, however, indicates little or nothing regarding the relative contribution of 'environment in general' to the production of 'individual differences in general,' since the observed inter-twin differences will themselves depend not only upon the specific nature of the environmental influences, but also upon the specific hereditary characteristics of the particular twins under observation. For example, a given environmental disparity might produce much smaller differences in the behavior of a pair of microcephalic identical twins than would occur if the twins had normal structural prerequisites for intellectual development. In other words, any estimate of the influence of 'environment' would be specific to the individuals and to the environments under consideration.

Another closely related approach is implied by the assertion that the influence of heredity becomes increasingly evident as environmental conditions improve and that the hereditary contribution would be at a maximum under conditions of 'optimum environment.' For example, as long as different groups of people are reared in communities which vary conspicuously in their educational opportunities, then individual differences in performance among adults in the total population are in part at-

[5] *Cf., e.g.,* Woodworth's (17) estimates of the percentage contribution of inter-family and intra-family environmental differences in IQ, in contrast to his clear expression of the interaction view in the same monograph as well as elsewhere. *Cf.* also Burks' (2) computation of the percentage contribution of heredity and environment to IQ by means of path coefficients, despite her statement (1) that, "Environment may have different degrees of influence when the endowment for a given trait is of larger or smaller amount."

tributable to such environmental differences. It is argued, however, that as educational facilities improve, a point is approached at which each individual is offered as much education as he is capable of assimilating. At this point, individual differences in adult performance would be attributed primarily to hereditary factors.

Such an approach involves a number of possible pitfalls. First, it frequently carries the implication that the operation of heredity had been merely 'obscured' by the environmental differences, and that the 'true' contribution of heredity stands revealed under optimum conditions of environment. This argument is of course inconsistent with the concept of interaction between hereditary and environmental influences. Thus from observations made in such an 'optimum' environment, we could not generalize to other environments on the grounds that we had isolated *the* contribution of heredity.

Secondly, it would be very difficult to define 'optimum environment.' One may well ask, optimum for what? There seems to be an assumption here of a preordained type of development, in terms of which the most favorable environment can be identified. Thus an optimum environment is often described as one which offers no special handicaps, obstructions, or interference with the 'normal process of development.' This suggests predeterminism in the genes, and is reminiscent of the notion of the homunculus in the fertilized ovum, whose latent characteristics merely 'unfold' if given the opportunity.

When we attempt to define optimum environment in a more specific manner, we find no single continuum of 'effectiveness' in terms of which environment can be graded. The optimum environment would vary with the specific result to be achieved, *e.g.*, high Stanford-Binet IQ, artistic aptitude, executive ability, originality, acquiescence, *etc.* If, now, we arbitrarily define optimum environment with reference to a specified objective, such as high Stanford-Binet IQ, a further difficulty is met in the differential effect of the same environment on different individuals. To take an hypothetical, oversimplified example, an individual who, because of certain hereditary biological factors, tends to be overactive might achieve his best intellectual development in an environment conducive to relaxation; for one who is underactive, the corresponding 'optimum' environment might be one conducive to excitement. Thus the optimum environment for the attainment of any given result will differ for each individual, unless it is assumed that behavior development is independent of any hereditary individual differences. If, however, the definition of optimum environment assumes the absence of such hereditary differences, one obviously cannot conclude that in such an optimum environment hereditary influences are at their maximum!

The Nature of Heredity

If pressed for a concrete definition of heredity, many psychologists will suddenly leave the area of behavior resemblances and differences which they have been studying, and with a quick change of scene introduce the biological mechanism of the genes. The genes are thus regarded as the mechanism for the inheritance of psychological traits, by a sort of analogy with their demonstrated rôle in the transmission of structural characteristics. Regardless of how heredity is defined, however, all psychologists would undoubtedly agree that the genes play an important rôle in their concept of heredity. But the exact rôle will vary in different concepts. Some will maintain that heredity is 'carried' by the genes or that it is 'determined' by the genes. Others will insist that heredity *is* the genes, thus defining heredity as the specific material with which the individual begins life at conception.[6]

From a realistic, objective point of view, the genes obviously consist of specific chemical substances. They are not filled with 'potentialities,' 'tendencies,' 'influences,' 'determiners,' or other mystical entities. As Jennings puts it, "That which is directly inherited . . . is the set of genes, with the accompanying cytoplasm:—certain substances in certain combinations, which under certain conditions give rise to the individual, having certain later characteristics" (8, pp. 133–134). Similarly, Holt (7, p. 9) writes: "No potential character ever is 'already contained' in anything: and the notion of potentiality, wherever used, is a mark of finalistic thinking. The contents of the germ-cell are not potential characters at all, whether bodily or mental: they are actual proteins and other substances, and to call these substances 'potential' this or that is to flout the truth."

The fact that adult individuals differ from species to species, as well as within species, is undoubtedly related to the specific chemical constitution of the germ cells out of which each individual developed. In the same sense, an iron knocker differs from a brass knocker because of the difference in the original material out of which it was fashioned. But it would be pointless to insist that the original piece of iron contained the potentialities of the knocker, or that as the result of proper handling by a skilled worker (*i.e.*, 'favorable' environment), its normal knocker potentialities were realized. It would have been equally 'normal' for the iron to become a horseshoe.

[6] *Cf.*, *e.g.*, Jennings (8), Holt (7), Chein (4).

THE NATURE OF ENVIRONMENT

Psychologists have not only been frequently remiss in failing to sharpen and clarify their concept of 'heredity' as applied to behavior phenomena, but have often been equally vague in their use of the term 'environment.' Recognition of the implications of this term is of basic importance for an understanding of the heredity-environment problem.

'Stimulational' versus 'locational' rôle. Environment has been all too frequently envisaged as a *passive place or 'locus'* in which the organism's behavior is said to occur. In other words, the environment is regarded as a setting for behavior, rather than as an *active stimulating agent*. The former, passive sense of the term seems to be that characteristically implied by sociologists as well as by many psychologists. Actually, however, from a psychological point of view the environment consists of a myriad of specific stimuli which act upon the behaving organism.[7]

Specificity. The layman's notion of environment is usually a rather general or superficial *geographical* one, as illustrated by such descriptions as a city slum, a suburb, or a French village. A somewhat more discriminating, *familial* definition is implied in the frequent popular assertions that any differences in ability, interest, emotional adjustment, and the like between siblings in the same home must be the result of heredity, "since the environments were the same."

An *individual* definition of environment recognizes the marked differences in personal relationships, participation in various activities, and the like, among

[7] Strictly speaking, of course, the physical characteristics of the organism itself will in part determine the effectiveness of the environmental stimulation, as will the organism's previous 'experience,' conditioning, or reactional biography. (*Cf.*, *e.g.*, Kantor's concept of stimulus function, 10, Ch. I, II, III.)

individuals in the same home. It is apparent that from a psychological point of view, environment must be regarded as a complex of stimuli which is unique for each individual. Finally, the consideration of *inter-cellular* and *intra-cellular* environment and their rôle in the processes of growth has further modified the concept of environment and has dispelled the notion of environment as an 'external' force in contrast to heredity operating 'from within.'

Temporal extent. Closely related to the scope and definition of environmental influences is the time of onset of such influences in the developmental cycle of the individual. The erroneous popular identification of heredity with that which is present at birth is reflected in the word 'native,' which signifies 'hereditary' but has the same root as 'natal' or 'pertaining to birth.' The experimental production of various monsters by the modification of the prenatal environment, as well as the extensive research on prenatal behavior development, has clearly disproved the belief that whatever is present at birth must be wholly the product of heredity. Through such experiments the starting point of environmental influences has been pushed back to the moment of conception. Moreover, experiments on the effects of radiation suggest that the genes themselves are susceptible to change in response to certain environmental agents acting even prior to fertilization. Thus the operation of environment appears to be co-extensive in time with that of heredity.

This brief examination of the heredity-environment problem suggests that the more precisely heredity and environment are defined and the more fully their operation is investigated, the more inextricably do they appear to be intertwined. Moreover, when heredity is stripped of mystical, intangible concepts and defined objectively in terms of specific chemical substances which constitute the genes, the connection between heredity and behavior appears extremely remote and indirect. The differentiation between heredity and environment is thus becoming not only increasingly difficult, but also of doubtful significance for the understanding of behavior. In fact, it might be argued that any attempt to abstract the relative contributions of 'heredity' and 'environment' becomes operationally meaningless owing to: (1) the enormous variety of specific hereditary and environmental influences; (2) the differences in their probable contributions to different behavior characteristics within the individual; (3) the differences in their probable contributions to behavior development in different individuals; and (4) the interacting nature of the operation of such factors.

Despite these almost insurmountable methodological obstacles, the issue which goes under the name of the heredity-environment problem is still very much alive. If evidence were needed for this statement it would be found in the 1940 *Yearbook of the National Society for the Study of Education* and in the protracted methodological controversy which has raged over the Iowa nursery school studies, as well as in the number of surveys and critical articles on the whole problem which have appeared during the past decade.

HEREDITARY-ENVIRONMENTAL VERSUS STRUCTURAL-FUNCTIONAL ANALYSIS

It is the thesis of this paper that most of the questions asked by psychologists regarding the etiology of behavior are not in effect concerned with heredity-or-environment, but rather with structural-or-functional factors. From the viewpoint of both the practical control of behavior and the stimulation of fruitful research, the real problem seems to

	Structural	Functional
Hereditary	A. Hereditary structural characteristics	C. Hereditary functional characteristics
Environmental	B. Environmentally determined structural characteristics	D. Environmentally determined functional characteristics

FIG. 1. Schema of hypothetical possibilities in behavior etiology

be an analysis of the dependence of specific behavior characteristics upon structural conditions on the one hand or upon the individual's reactional biography on the other. The confusion of the 'structure-function' dichotomy with the 'heredity-environment' dichotomy probably underlies some of the sharpest disagreements in this field.

Theoretically, we may recognize four possible combinations among these categories, as illustrated in Fig. 1. As applied to the etiology of behavior, Class A comprises those instances in which an inherited structural characteristic precludes the attainment of a specific type or degree of behavior development, as illustrated by intellectual defect associated with amaurotic juvenile idiocy.[8] Mental defect resulting from cerebral birth lesions represents an example of Class B. Class C is implied in assertions that there are specific genes or gene combinations corresponding to the condition of feeblemindedness *per se*. It is also implied by statements regarding the inheritance of artistic talent, mathematical aptitude, criminal tendencies, and the like. Class D can be illustrated by intellectual defect resulting from inadequate opportunity to learn, as in the frequently cited canal boat children, isolated mountaineers, and similar groups. In such cases, the deficiency is attributed to conditions in the individual's previous reactional biography.

It will be noted that the terms 'structural' and 'functional' as used in Fig. 1 and throughout the present discussion actually refer to biological structure on the one hand and psychological function on the other. The latter is distinguished from biological functioning, which depends upon the specific properties of the structures and is more inexorably determined by such structures.[9] Given a certain type of digestive system and food, for example, digestion will occur. But given normal human vocal structures and the auditory stimulus, "How are you?", the individual will not necessarily reply, "Fine, thank you." Depending upon his reactional biography, he may respond with, "Excuse, I speak no English," or he may merely stare in open-mouthed apathy, or possibly punch his interlocutor on the jaw. The structural-functional distinction as herein used is similar to the traditional usage of 'organic' and 'functional' in the classification of psychoses. In such a classification, 'organic' refers to the presence of specific

[8] This is one of the most clearly established instances of the rôle of hereditary structural characteristics in the development of feeblemindedness. A simple recessive factor is generally believed to be responsible for this condition. Even in this case, however, knowledge of the exact etiology is quite imperfect and the evidence regarding the hereditary factor still tentative.

[9] For a further discussion of the distinction between psychological, biological, and physical functioning, *cf.*, *e.g.*, Kantor (9, Ch. I; 10, Ch. I and III).

structural deficiencies underlying the aberrant behavior; 'functional' denotes behavior disorders with no such structural deficiencies. The structural defect in organic psychoses may, however, be hereditary or environmental in origin. The distinction is not, therefore, one between inherited and environmental etiology.

Furthermore, the term 'functional psychosis' does not refer to improper *biological* functioning of any organ system. On the contrary, the term 'organic' is used to designate any defect either in structures or in their corresponding biological functions, *i.e.*, either anatomical or physiological. In the same sense, 'structural' has been used in the present discussion to denote characteristics of bodily structures or of their intimately related biological functions.

What we have designated for brevity the 'structural-functional' dichotomy is at once theoretically more tenable and of more frequent practical significance than the heredity-environment distinction. In the treatment of individual cases, for example, it is of prime concern to know whether a behavioral abnormality results from structural factors on the one hand, or from such conditions as previous experience, inadequate schooling, or socio-economic level, on the other. The frequent use of the term 'constitutional,' which straddles Classes A and B in Fig. 1, further illustrates the common reliance upon the structure-function classification.[10]

For the research psychologist, attempts to study the operation of heredity are likely to lead to fruitless or ambiguous experimental design. Reformulating the questions in terms of structural influences, regardless of the hereditary or environmental nature of the latter, would probably be a more heuristic approach. The geneticists could then tell us which structural characteristics have proved to be transmissible through the specific substance of the genes. This, however, is by no means synonymous with the statement that any one behavior characteristic is so transmissible.

A good illustration of the intertwining of heredity and environment in the structural factors which underlie behavior characteristics is furnished by recent research on the Rh factor in the blood. Among a certain percentage of 'undifferentiated feebleminded,' not classifiable under any of the traditional clinical categories, the Rh factor was found to be negative in the mother and positive in the child (*cf.*, *e.g.*, 5; 14, Ch. IX; 15). These percentages have been reported to be significantly in excess of chance in most investigations to date. Geneticists have suggested that through the transfusion of blood which normally occurs between the mother and the embryo, the Rh incompatibility may produce a condition of insufficient oxygen; if this, in turn, occurs at a critical stage of brain development in the embryo, feeblemindedness may result. Thus, although the Rh factor is hereditary, such feeblemindedness would not be hereditary, but would be acquired by the embryo as an *environmental* effect. The investigator interested in determining whether 'constitutional factors' or 'opportunity to learn' were of prime importance in a particular case, however, would hardly classify the Rh factor under the latter!

A similar point may be made in reference to cerebral birth lesions. These are clearly environmental factors, but little would be gained by classing them together with socio-economic level and nursery school attendance. Furthermore, it could be argued that heredi-

[10] Those who believe that disembodied functions can be transmitted through the genes would also include Class C under the term 'constitutional.'

tary factors probably play a part in the development of such characteristics as cranial conformation in the child or pelvic dimensions in the mother, such conditions in turn influencing the likelihood of cerebral birth injuries. Thus with reference to behavior development, heredity may enter into the operation of such 'environmental conditions' as birth lesions, and conversely, environment may be involved in the operation of such 'hereditary influences' as the Rh factor.

It should be noted that the most common source of disagreement between 'hereditarians' and 'environmentalists' in psychology pertains to Class C in Fig. 1, *viz.*, 'inherited functional characteristics.' We may consider, for example, the widely quoted and much maligned statement by John B. Watson: "Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant-chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors" (16, p. 104). In the light of its context, it appears very likely that this statement represented only a protest against the type of behavior etiology indicated by Class C. The statement might thus be paraphrased to read: "Given a group of children with normal structural prerequisites, any behavioral variance among them can be attributed to their respective reactional biographies." Much confusion has resulted from the failure to realize that the terms 'healthy' and 'well-formed' in Watson's statement implied freedom from any structural deficiencies that might be relevant to behavior development.[11] It was the inheritance of psychological functions as such that this statement rejected.

Many allegedly 'extreme hereditarians' would probably agree that no behavior as such is inherited, and that they are merely arguing for the importance of Class A and Class B factors, as contrasted to Class D factors, in the etiology of certain specific behavior characteristics. Thus the more explicit formulation of the problem immediately reveals the superficial nature of the disagreement.

The proposed analysis of behavior etiology into structural-and-functional rather than hereditary-and-environmental factors does not imply any attempt to 'reduce' psychological functions to biological or physical ones, or to 'explain' one in terms of the other. Explanations of behavior must of course be sought among behavior phenomena themselves. It is well known that attempts to 'explain' psychological phenomena in biological terms have often produced only animistic notions in a pseudo-neurological guise. This matter has been fully discussed by Kantor, who concludes: "Not only must we not regard psychological conduct as sheer biological activity, but also we cannot look upon the biological concomitants of psychological responses as the causes of the latter. . . . Probably the most effective way is to consider the biological factors as participants in the psychological response—namely, the operations of the biological mechanisms are factors in a psychological event" (10, pp. 49–50). In so far as structural factors are 'participants,' however, their study will aid in the understanding of behavior by furnishing a more complete picture of the condi-

[11] To be sure, Watson's statement seems to imply that structural characteristics must be 'pathological' in degree in order to influence behavior development. He does not appear to consider the possibility that normal variations in structural characteristics within the species may limit behavior development in particular directions.

tions under which specific behavior characteristics appear.

In summary, it is proposed that the etiology of behavior be approached in terms of structural contributions versus the contributions of reactional biography, rather than in terms of the traditional heredity-environment dichotomy. It has become increasingly apparent that the operation of heredity is inextricably linked with that of environment. Moreover, since heredity must necessarily operate through the medium of structural factors, it follows that the applicability of the concept of heredity to behavior phenomena is indirect and remote. In the light of these theoretical considerations, as well as from a heuristic and a practical point of view, the structural-functional analysis of behavior appears to be more productive than that in terms of heredity and environment.

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THE DOCTRINE OF SUGGESTION, PRESTIGE AND IMITATION IN SOCIAL PSYCHOLOGY[1]

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We have today in social psychology a far-reaching and widely adopted theory of the effect of group forces on the formation and change of opinions and attitudes. At its center are the concepts of suggestion, prestige and imitation—terms which in social psychology are virtually interchangeable. On the basis of this theory there has developed an empirical approach to the investigation of group influences on processes of evaluation. Employing this approach numerous investigators have reported results which appear to be in substantial agreement, and fundamental conclusions have been drawn concerning the psychological processes in question which appear to support the initial assumptions. It is the purpose of the present paper to submit this procedure to a careful examination, in order to inquire into some hitherto unrecognized difficulties in its presuppositions, and to raise certain questions concerning the adequacy of the theoretical formulations.

It is necessary first to become clear concerning the range of social facts in relation to which the processes of suggestion, prestige and imitation were first described. There can be little doubt that the factual basis was in those social actions and opinions that appear to possess a blind character. The observation that individuals and groups hold and defend views not based on adequate knowledge, that decisions are made and actions taken which have little to do with the actual merits of the situation, and that group forces can produce extraordinary effects in contradiction to the most elementary demands of reason and even of self-interest—facts of this order have constituted the basis of suggestion-doctrine in social psychology. These observations have seemed to gain added importance in recent times when the production of such effects has been harnessed and institutionalized in the form of mass propaganda and advertising. That groups can be whipped by propaganda into a condition of excitement to the point where they see the issues only in the manner that they are posed to them has been taken as confirmation of the influence of these massive and often ugly facts.

It is in relation to facts of this order that the concepts of suggestion, imitation and prestige were developed. The facts themselves seemed clear; beliefs and attitudes could be observed that were inadequate to or contradicted the actual demands of situations. The task of psychological inquiry was to discover the psychological processes responsible for such effects. The answer took a simple form: A process was described which was capable of inducing people to accept arbitrarily opinions and evaluations regardless of their merit. The concepts of suggestion, imitation and prestige appeared to meet this need in one essential regard: They transferred the blindness observable in the consequences of social action into the psychological process itself.

We cannot here trace the manner in which the concepts in question were extended to the entire range of social events. It is sufficient to record the

[1] The present paper was begun when the writer was a Fellow of the John Simon Guggenheim Memorial Foundation.

fact that the doctrine of suggestion, originally oriented to the formation of social *misconceptions*, has been given a general application, and has become the theory of the formation and change of opinions and attitudes. In consequence the psychology of attitudes is well-nigh universally (both in social psychology and in the social sciences generally) treated as at bottom an affair of suggestion and bias. This approach has penetrated nearly all regions of social psychology and has determined in a far-reaching way not only the mode of thinking but also the formulation of problems and the details of investigation. It would seem at present that the issue has been settled, and that the task of investigation in the future is to supply more and more refined support for an established principle.

It will be the thesis of the present paper that the case for the doctrine of suggestion has not been proven. We shall attempt to show that investigations in this region do not support the conclusions that have been drawn from them, or the assumptions upon which they were based. We shall present evidence to the effect that the investigations and the theoretical formulations have dogmatically excluded a range of facts of great importance in which processes of understanding play a clear and important rôle, and further, that the concepts may be inadequate even to the range of arbitrary action and judgment. This we shall do by an examination of certain representative studies in this region. In order to establish these conclusions it will be necessary to show that the procedures employed in the investigation of social judgments have often introduced grave distortions of interpretation, that the conclusions drawn have often been at variance with the evidence, and that the experimental approach has been employed one-sidedly to support a theory which has simply been assumed but not itself subjected to investigation. We shall deal here with the wider theoretical consequences only insofar as is necessary for our specific aim, which is to inquire whether the conclusions drawn from the current approach are consistent with the evidence or even with its own assumptions.[2]

Before proceeding with our examination it may be helpful to note two preliminary points. When we refer to actions not determined by the merits of a given situation we are at the same time implying that there are actions differently determined. One cannot speak of 'uncritical' action without assuming actions that are not uncritical. It is therefore appropriate to ask whether 'critical' action need not be studied in its own right, and whether it is sufficiently characterized (as investigation and theory have done, at least implicitly) by the absence of factors of suggestion. To do so would appear inadequate, just as it would be inadequate to describe thinking as the absence of errors in thinking. Even if the validity of the process of suggestion were to be assumed, there would then still remain a serious incompleteness in its application to social facts.

It seems also necessary to mention a curious difficulty that confronts the student dealing with theoretical issues in social psychology at the present rudimentary stage of investigation. The phenomena of which one speaks possess qualities and relations that are immediately understandable in terms of our daily experience. Nevertheless, these qualities and relations may not be represented, and may even be denied, in the theoretical formulations. Few, for example, would care to assert bluntly that understanding plays no rôle in the

[2] A more detailed account will appear in a forthcoming work by the writer on social psychology.

social sphere. Yet this is precisely what psychological theories often do assert. When calling attention to these matters one appears to be charging investigators with failure to make distinctions that are obvious and of which they surely could not have been unmindful. It may then appear that one is setting up a theory which was not that of the investigator in order to oppose it. Thereupon the temptation arises to read the theoretical formulations in such a way that they will not contradict the ordinary facts of observation. In this direction there lies, in the opinion of the writer, a serious danger. If the aim is scientific clarification, there is no way other than to examine in strictness what a proposition asserts, and to consider as irrelevant the fact that it may be at variance with the insights or the intentions of its authors. It should not be surprising that in the regions of social psychology, of which we possess an intimate acquaintance by familiarity, our ordinary insights should daily outdistance our first efforts at theory.

I. The Problem and the Technical Procedure of the Current Approach

Investigation in this region starts from the observed fact that the evaluation by a person of issues of the most diverse kind can be altered by the knowledge of the manner in which they have been judged by certain groups or persons. The aim of investigation has been to demonstrate under controlled conditions that such changes in evaluation can be produced, to measure them quantitatively, and to explain them in terms of a consistent theory.

The concrete procedure of investigation has followed a rather uniform pattern. Considered solely from the technical side there is a simple idea at its basis. The subject states a judgment concerning an issue; this may concern a political, aesthetic or economic problem. At a later time the same subject again judges the same problem; this time, however, he is also informed of the manner in which certain groups or noted persons have evaluated the problem. If, on the second occasion, the subject alters his original judgment in the direction of the group or person in question, the change is taken as a measure of the degree of influence the latter has exerted on his judgment. The comparison of evaluations under the two conditions is the aim as well as the ground for conclusions concerning the psychological character of the process. The procedure described is entirely cleaг and contains little that is striking. There is the observation of an event before and after the introduction of an experimental factor, and the quantitative comparison of the results under the two conditions.

Investigations employing this approach have been reported by Zillig (9), Moore (4), Marple (3), Sherif (6), Thorndike (7), Lorge (2), and others. Though these studies raise different and interesting problems, space forbids their detailed discussion. We have, instead, decided to submit to detailed examination a few investigations that may be considered representative. It seemed desirable to select for this purpose studies which represent the procedure at its best.

II. The Investigation of Lorge [3]

We select first for examination the investigation of Lorge which deals with the effects of 'prestige' on the evaluation of statements touching serious economic and political questions. It is necessary to state at the outset that it was not the aim of Lorge to formulate explicitly a

[3] The writer wishes to express his appreciation to Professor Irving Lorge for his kindness in making available certain original data on which a portion of the following discussion is based.

theory of prestige. As will become evident in the following account, the main object was more limited and factual, namely, to demonstrate by a quantitative procedure the presence of a factor of prestige. We choose it for discussion because it is among the more significant and careful investigations in this region; it deals with issues of a serious social content and is an extensive and carefully executed work.

The starting point of the investigation is the observation that a factor of 'prestige' is capable of altering the evaluations of statements concerning serious political and economic questions. It attempts to measure quantitatively the effect of this factor. The initial assumption is that a person, confronted with an opinion from one who has prestige for him, will have his reaction to it colored accordingly.

The investigation was conducted in 1934 (during the depression) with a group of 99 unemployed adult subjects, working at the time for the Emergency Relief Bureau of New York City and participating in the experiment as part of their regular duties. Their education was above average; and their intelligence was, according to Lorge, superior. The general plan was to compare the quantitative ratings of a series of identical statements under two conditions of identification. All statements were evaluated twice, and most were referred each time to a different author. The detailed procedure is slightly complex, and must now be described in its several steps.

The subject rated each of a set of 50 brief quotations on a 5-point scale, indicating the degree of his 'agreement' (or 'disagreement') with it. Each quotation was followed by the names of two public persons (or of newspapers, or of political groups), one of which was the true source. The subject was also instructed to select the true author of each passage from among the two names (having received the information that the name of the actual author was in each case included). Subsequently, after a lapse of two weeks to a month, the subject again rated the identical quotations. This time each quotation was followed by only one name, that of the true author. Some time prior to the ratings of the statements the subject had rated, again on a 5-point scale, the names of the persons, publications, organizations with whom the quotations were later identified. These ratings were in terms of the subject's 'respect for the political opinions of each of these individuals.' This was the measure of prestige.

Some of the statements were attributed to an incorrect author during the first evaluation. But in the course of the second evaluation these were all correctly identified for him. This condition, which introduces in effect a change of authorship between the two occasions, permits the study of the effect of the latter factor upon any changes that may have occurred in the corresponding ratings of the statements.

We illustrate with one concrete example the procedure and the first step in the analysis of the results. The subject reads and rates the following statement, with the added instruction to select the true author from one of the names appearing after it:

> "Those who hold and those who are without property have ever formed two distinct classes."
>
> Karl Marx
> *John Adams*[4]

Let us suppose that the subject incorrectly chooses Marx as the author of the statement he now rates. Several weeks later he again reads the state-

[4] The true authors are designated here and throughout the paper in italics. They were, of course, not distinguished in this way in the investigation of Lorge.

ment, this time with the information that the author is John Adams, and again he rates it. In the analysis of the data, Lorge proceeds as follows: The subject had assigned

(1) to Marx a rating of 5; [5]
(2) to the statement when attributed to Marx a rating of 4;
(3) to John Adams a rating of 3;
(4) to the statement when attributed to Adams a rating of 2.

Therefore, a difference of + 2 in the ratings of the authors is accompanied by a shift of + 2 in the ratings of the statement. Lorge concludes that the difference in the rating of the authors —or the positive difference of prestige— was the factor responsible for the drop in the rating of the statement. Each statement is treated in this manner. The final step in the analysis, and the goal of all the preceding operations, is to relate the changes in the evaluations of the statements to the changes in the ratings of the authors or to obtain the relation of (1)–(3) to (2)–(4) for the entire group.

Of necessity the final calculation is somewhat complex. Not all subjects identified or rated the statements in the same way. It is therefore necessary to apply a common statistical treatment to all the changes. This is accomplished by taking the difference of author-ratings as the basis of calculation and by grouping all responses with reference to the size of the latter. For example, the instance cited above would be grouped with all other responses which involved a shift of + 2 in the rating of the authors. Since the possible differences in ratings of authors range between − 4 and + 4 there are 9 such categories in all.

It should be noted that identical values can be obtained between different points on the rating scale. For example, the category − 3 will include differences in ratings of authors from 5 to 2 and 4 to 1.

Having categorized the responses in this manner, Lorge computes the amount of shift in the ratings of the statements in each category in terms of the mean and the median. The amount of change is taken as the average effect produced by the change of prestige represented in that category. Finally, the effects obtained in the different categories are compared.

When the data are treated in the manner described, the following group results are evident: (1) The subjects tend to rate the same statement differently when it is referred to a different author. (2) More specifically, the changes in the ratings of the statements correspond in direction to the differences in the ratings of the authors. That is to say, the rating of a statement tends to rise when it is referred to a more highly regarded author. (3) Further, the amount of change in the rating of a statement tends to vary positively with the amount of difference in the ratings of the authors. (4) Finally, negative changes in the evaluation of a statement consequent upon negative changes in the prestige of the author tend to be quantitatively smaller than positive changes in response to positive changes in the prestige of the author.

On the basis of these findings the interpretation is proposed that a change in prestige produces a change in the approval of the statements, and that the size of the change is determined by the quantity of prestige. In the words of the study: "The difference between the ratings (*i.e.*, of the statements) . . . can now be considered in terms of the

[5] In the scale consisting of 5 steps, '1' represents the extreme of agreement or regard, while '5' represents the negative extreme. Lorge designated the steps on the scale in a slightly different manner, which is not, however, of consequence for the present discussion.

difference in regard for the sources of the quotations at the two successive administrations" (2, p. 394). With regard to the greater size of changes in the direction of positive prestige, an interpretation is offered in terms of the 'law of effect.' It is proposed that high and low prestige functioned in the investigation as rewards and punishments in the sense of the law of effect, the consequences of which are claimed by Thorndike and his associates to differ just in this direction.

III. Two Interpretations

Before turning to the detailed examination of the procedure, we shall attempt, in this section, to clarify the theory of prestige underlying it, and to confront it with an alternative interpretation. That the relating of an action or a policy to its source has consequences for evaluation seems clear. What remains to be clarified, however, is the specific character of the processes in question. What is to be understood by the proposition that prestige modifies judgments and evaluations? What is the factor of prestige and how does it function?

We shall illustrate with the following statement which, though it was not included in the investigation of Lorge, is adequate to the purpose:

"Only the wilfully blind can fail to see that the old style capitalism of a primitive freebooting period is gone forever. The capitalism of complete laissez-faire, which thrived on low wages and maximum profits for minimum turnover, which rejected collective bargaining and fought against justified public regulation of the competitive process, is a thing of the past."

Thesis I. Attachment of prestige. Let us take the case of a subject who initially read the statement on the incorrect assumption that its author was Harry Bridges, the well-known union leader, and that he read it again after he was informed that the actual author was Eric A. Johnston, at the time president of the U. S. Chamber of Commerce. This subject had assigned

(1) to Bridges a rating of 5;
(2) to the statement when attributed to Bridges a rating of 4;
(3) to Johnston a rating of 1;
(4) to the statement when attributed to Johnston a rating of 1.

As described earlier, it would be concluded that the higher prestige of Mr. Johnston (an advantage of 4 points on the scale) had the effect of increasing the 'liking' of the statement to the extent of 3 points on the scale.

What are the essential features of this interpretation? It contains the following factors:

(1) a source of high prestige, which we may call P +;
(2) a source of low (or lower) prestige, P −; and
(3) an object of judgment, the statement S, which would, standing alone, presumably lead to the evaluation X.

When S is brought in contact with P + (or P −), the evaluation X is said to increase (or diminish) in the direction of P + (or P −).

$$\text{Statement S} \longrightarrow \text{Evaluation X} \begin{matrix} \nearrow \text{P+} \\ \searrow \text{P}- \end{matrix}$$

The basic proposition of this theory, and one which is generally presupposed in investigations of prestige, is that *an unchanged object of judgment undergoes change of evaluation.* The content is assumed to remain constant, while the evaluation of it changes. Referring to our example, the subject's endorsement of the 'old style capitalism of a freebooting period' is taken to undergo change in accordance with the prestige acting on it. This proposition is clearly formulated in the following passage: "The inference that an increase in esteem for the true author over that in which the presumed author is held will result in higher ratings of the quotations *regardless of the merit of the quotations,* is consistent with the facts and reliably demonstrated" (2, p. 399; italics are ours).

The heart of the proposition is that *the material content of the object of judgment plays no rôle* in the process of change. The relation of the assertion to its author is assumed to be—and, to be consistent, it must be—strictly indifferent to the content of the statement. The prestige of the author enforces—and must enforce—its effect equally upon *any* statement to which it is attached, including contradictory statements.

The process at work is, in fact, brought into relation with the law of effect of Thorndike. Just as in the formulation of the law of effect a reward or punishment is conceived to be arbitrarily attached, by fiat of the experimenter, to any response, so in the present investigation prestige is viewed as acting arbitrarily upon a statement, regardless of its content or merit.[6]

Thesis II. The cognitive change of content and function with change in context. We propose an interpretation of the psychological process fundamentally different from the one described. First, it seems necessary to insist that the evaluation of an act or an assertion depends in the first instance on what it is understood to do or say; the evaluation is necessarily a function of its content. Secondly, the content of a given act or assertion is not as a rule a fixed entity; generally, it is perceived as part of a wider setting, referring to what has preceded or what is to follow. The specific content of an event or utterance is a function of the perceived relation between it and its context. Given an assertion in a particular context, the person will perceive it as part of this context, in terms of what it says and does in this place. Only then will the evaluation follow. It is, therefore, a first requirement of method to establish the actual content for the subject of the material to which he is addressing himself, and further, to observe how the content is determined by its setting. Proceeding in this manner, we reach, as will be indicated subsequently, an account of the psychological processes that radically departs from and contradicts the assumptions of the first approach.

In the course of an earlier investigation (1, see especially pp. 456–458), we presented evidence that under certain conditions a 'change of judgment' in response to group standards could not be interpreted in terms of the first conception, or in terms of suggestion or prestige. Based on an examination of the content of the evaluations, the evidence pointed to the presence of a different process, consisting in a redefinition of the object of judgment. The conclusion was there drawn that there occur apparent changes in evaluation which are due to "*a change in the object of judgment, rather than in the judgment of the object*" (1, p. 458; italics in the original).

More directly relevant to the present discussion is a second investigation, which we shall briefly illustrate.[7] The procedure was, in comparison to that generally employed, much simplified.

[6] In the course of a personal discussion Dr. Lorge has pointed out that he did not intend to question that changes of evaluation due to changes in the material content of the object of judgment occur. It seems nevertheless clear that the factor of prestige as studied in the investigation did not include a reference to such changes.

[7] A detailed account of this investigation will be published shortly.

The subjects, all college students, read a number of statements in succession, each accompanied by the name of one author, under instructions to describe in their own words "what the statement means." There were two groups of subjects, each of which read the same passages, but with the names of different authors attached. Included among them was that of Mr. Johnston cited above. Below are reproduced some of the reactions of the subjects:

Bridges

"Bridges is saying that there is not much possibility of exploiting labor today as in the past. He implies that the power of business has to yield to the power of unionized labor and of public opinion. That is a true statement of fact, though I rather wish it were not. I do not believe in the many radical unions, though I realize that unions sometimes do some good."

"I interpret this to be a statement against any attempt to overthrow labor's powers. . . . Mr. Bridges is expressing his opposition to all capitalist attempts to change the existing trends."

"Bridges has fought for labor all his life, and in this statement he embodies the core of all that labor is fighting for."

"Probably from a speech trying to convince some people that they ought to join a union."

"I believe that this is a prediction of the end of big business."

Johnston

"Mr. Johnston, representative of many businessmen of this country, realizes that if capitalism is to continue, it will have to be enlightened capitalism."

"Mr. Johnston seems to believe that modifications of the extreme competitiveness of capitalism are inevitable and also desirable."

"I agree completely. The question which logically follow is: How much government control of business?"

"Mr. Johnston hardly talks like a president of the Chamber of Commerce. He speaks rather like a liberal-minded citizen who has seen the faults in a system of complete laissez-faire."

"This doesn't sound like a member of the Chamber of Commerce."

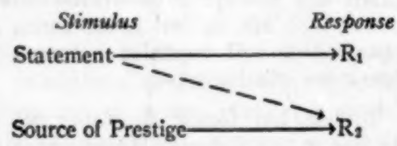
We cannot here enter into detail concerning the significance of these reactions, which are quite representative. It is sufficient for the present purpose to note that a cognitive reorganization of the statement in relation to what was understood of its author was the most significant feature of the responses. When referred to Bridges the content of the passage turned into an expression of the accomplishments of labor in the face of opposition from capital, and contained a resolve to defend these gains from attack. On the other hand, when attributed to Johnston it was read as a perspective of policy in the interest of business, especially of 'enlightened' business. Of considerable interest also is the rejection of Johnston's authorship by 6 of 35 subjects, of which no instances occurred when Bridges was the presumed author. The presence of such reactions is further evidence that, at least in certain cases, we are not dealing with the automatic effects of a factor of prestige on evaluation, a fact which has consequences for our understanding of the nature of prestige. These findings force a reinterpretation of the factual situation and of the theoretical problem. We shall subsequently present evidence in greater detail for the conclusion that the authorship of a proposition functioned generally not as a source of prestige but as a context for the determination of meaning. It is questionable whether the investigation of Lorge and related investigations dealt at all with a factor of prestige in the traditional sense. It seems more probable that they have unwittingly investigated the rôle of social knowledge and understanding.

IV. The Two Interpretations Compared

Though we are concerned here principally with the clarification of a particular problem, namely, the methodological adequacy of a certain mode of experimentation, it may not be amiss to mention that it touches upon wider issues in social psychology, and in general psychology as well. The approach of the first theory is part of a larger point of view in social psychology which denies the distinction between action and opinion determined by suggestion (which is the essential meaning of prestige) and that determined by intrinsic factors in the given situation. Characteristic of it is the tendency to reduce events of the latter kind to terms of the former. It starts with the axiom that attitudes and judgments are an affair of suggestion, and it reduces the most diverse happenings to this form. In contrast, the second approach affirms the distinction, and insists that judgment and evaluation determined by cognitive factors be not dogmatically excluded in advance from psychological investigation. Further, it questions whether even arbitrary social happenings can be adequately understood in terms of the current procedure. We shall attempt below to sketch some of the main differences between these two viewpoints.

1. For Thesis I the starting point is the functioning of isolated psychological processes. There is an item—a statement or an action—and there is, independently of it, a source of prestige. When brought together one exerts an effect on the other. Which items may be joined or kept apart is a matter of indifference; the theory allows—in fact, it requires—that the relation between them be strictly contingent. Content of statement and prestige of author are viewed as two independent factors which may be attached or detached at will.

2. The starting point in isolated psychological functions leads with necessity to the assumption of an arbitrary process in the interaction between them. When statement and source of prestige are joined, the response to the latter is said to force a modification of the response to the former. The resulting effect cannot be a function of the quality of the prestige in question and of the content of the neighboring stimulus in their relation to one another. The prestige stimulus is, so to speak, ready to exert its effect automatically upon whatever situation is brought into contact with it, while the situation is ready to be modified by any factor of prestige. The conceptual schema approximates that of earlier conditioning formulations, with the exception that neither of the responses is regarded as 'unconditioned.'



3. At the root of the preceding propositions is, as we have seen earlier (see pp. 255–56), the assumption of the fixed content of psychological processes. Starting with the view that there is a constant relation between a local stimulus and response, processes of change are understood in terms of fortuitous interactions between constant elementary processes. Concretely, for Thesis I the statement S remains the identical stimulus under the changed conditions, the only change allowed being that of a response (or of the strength of a response) to an unchanging stimulus. Its basic terms are: (1) *stimulus* (statement, source of prestige), (2) *response* (evaluation of statement, or of the source of prestige), and (3) *change of*

connection between stimulus and response. In short, Thesis I commits the 'constancy error' when it presupposes the fixed content of the stimulus.

4. There is also included in Thesis I the far-reaching assumption that *emotional* forces, by definition radically divorced from cognitive processes, compel changes of evaluation. The 'liking' of an assertion is said to be determined not by what it says, by its merits, but by the 'liking' for its author. Consequently, the process of change of evaluation is treated well-nigh exclusively as an expression of *bias* established by blindly working social forces. It is in this sense that Lorge views his investigation as dealing with 'susceptibility to propaganda.'

In opposition to these formulations, Thesis II asserts:

1. The psychological processes whose interaction is under discussion are from the outset in intimate relation, and not discrete entities. The content of prestige cannot be at all understood except in relation to actions and utterances which form its basis; nor do actions and assertions in the social field possess a univocal content regardless of their setting. That for purposes of investigation it is necessary and desirable to separate the phases of a unified process is no reason to deny their unitary character.

2. If the content of prestige is at all determined by the perceived qualities of action and character, then the way is open for the understanding of reasonable relations between them when these actually occur. By a reasonable relation in this context we refer to the *relevance* that obtains between the perception of a particular merit in the actions of a person or group and the kind of appreciation or trust we accord him. A process of this order possesses the property of 'following from,' of one thing being caused by another. We find it in the everyday instances when a person is admired for his sportsmanship, when another is respected for his courage, or when still another is feared and despised for his cruelty. It is not necessary to assert in terms of Thesis II that all relations between action or character and prestige are of this relevant order; the significance of the formulation consists in not excluding it from the outset as one possibility.

3. In connection with the specific issue under discussion, Thesis II asserts that an action or assertion in the Context *A* is different in content and function from an action or assertion in the Context *B*. In short, Thesis II denies the fixed character of events regardless of changing context. Accordingly, the basic terms of Thesis II are the processes taking place between a *part* and its *context*, processes that result in changes of content and function.

4. Finally, Thesis II questions the exclusion of cognitive processes from an understanding of emotions. Instead of assuming their fundamental separation, and consequently their later connection by mechanical means, it asserts that emotions are often determined in their content and course by cognitive processes, and that the relation between them may contain features of relevance. In this manner it seeks to establish a distinction between emotional processes that are appropriate and those that lack appropriateness. The development of appropriate prestige would be one illustration of such a process.

With regard to the particular problem under discussion the two theses lead to fundamentally different interpretations and predictions. In the widest sense the difference centers around the rôle of understanding in the social field. Thesis I excludes factors of understanding from the start, or reinterprets them in such a manner that they no longer retain the character of thinking processes. There lurks behind Thesis I the silent axiom that processes of social evaluation are as a rule irrational. On the other hand, for Thesis II the question of what was understood (or where understanding failed) remains central for investigation in social psychology.

We may illustrate some of the differences in connection with the Lorge investigation:

1. A change of evaluation in response to a change of context is interpreted in Thesis I as a result of emotional bias, it being assumed that were the subjects 'logical' their evaluations would have remained fixed as the context changed. The proposal of Thesis II is that the subjects were often acting in an eminently reasonable way when they reinterpreted the content of an assertion in relation to an altered context.

2. A constancy of evaluation despite changes of context would, in terms of Thesis I, presumably be evidence of 'critical' thinking. But the same result would, in accordance with Thesis II, lead to the question whether the fixity of response may not be evidence of rigidity or lack of understanding.

3. Rejection of the authorship of a given statement may, in terms of Thesis II, be evidence that the subject is actively attempting to establish relations of relevance between the parts of a situation and that he is not bending slavishly to the forces brought to bear upon him. Thesis I seems not capable of dealing with such instances, or would be forced to explain them in advance as solely the result of previously established prestige effects.

4. Finally, in terms of Thesis II, the results of the investigation—assuming that they are valid factually—are a secondary consequence of the reorganized cognitive content, which is the precondition for the altered evaluations. But the first thesis has not adequate means of dealing with the fact of reorganization, or with actions that depend on the organizational properties of situations.

The alternative view here proposed may be more clearly grasped, and possible misunderstandings of it might be avoided, by indicating what it does not assert. (a) It does not claim that cognitive processes insure the objective validity of evaluations and decisions. Processes of the kind described may lead to invalid conclusions if, to mention only one factor, the factual material is meager or misleading. (b) Nor do we intend to assert that cognitive processes exclude the operation of bias. Our claim is indeed more limited. We wish to assert that even when there is bias the specific process of evaluation is not without meaning, and is not wholly blind. Even in the case in which the context is wrongly understood, or in which a wrong context is introduced, the process still possesses a certain direction, namely, the relating of a part to a context. (c) It is not necessary for our present purpose to deny that prestige in the traditional sense is a reality. We have attempted only to show that such processes have not been observed in the investigations under discussion. (d) It might be supposed that prestige in the traditional sense was involved at an earlier step, namely, during the formation of the attitudes toward the groups and authorities which play such a considerable rôle in the shifting of evaluation. While it is not necessary to prejudge this statement, it may not be amiss to point out that by pushing the operation of prestige back to an earlier point the problems we have raised are not abolished. Indeed, the same issues reappear and would need to be faced with regard to the new problem.

V. A REEXAMINATION OF THE INVESTIGATION OF LORGE

It might appear that our examination is at an end. We have presented evidence which contradicts the usual interpretation of the process of evaluation and an alternative approach has been sketched, the further elaboration of which would seem to be in order. Nevertheless we shall not continue here with an account of the questions raised by Thesis II. Instead we shall return to the investigation of Lorge for the purpose of examining its technical structure in detail. This task seems to be necessary for two reasons. It is not without importance to throw light on the presuppositions and methods of a widespread mode of investigation. Further, insofar as certain technical details of the present procedure are likely to be retained in future investigation, it seems appropriate to consider their possibilities and limitations.

A. An unexpected relation of relevance between utterance and source of prestige. It has been stated that Thesis I presupposes an arbitrary relation between the force of prestige and the material upon which it works. It requires only a source of prestige and *any* content upon which to act. It is therefore of interest that despite this assumption the investigation under examination contains—as nearly all investigations in this area do—an outstanding relation of appropriateness between the contents to be evaluated and the source of prestige to which they are attached. Propositions of a social content are referred in this study to *political* figures and groups. They are not attributed indifferently to musicians, or poets, or athletes.[8] In terms of the theory, such a precaution was superfluous; the results should be equally effective in the one case as in the other. Already in this self-imposed restriction there may be implied an important assumption concerning the function of prestige, having to do with its cognitive content. The effect is to enhance the significance of the investigation. But we may not overlook the possibility that the practice may be superior to the theory. A strict test of the theory would require that intrinsic relations between material to be evaluated and sources of prestige be, as far as possible, excluded.

In fact, additional relations of relevance between statement and source are included in investigations within this area, which do not ordinarily obtrude themselves precisely because of their naturalness. That the statements chosen for evaluation are not incomprehensible, that they are not as a rule absurd, testifies to certain implicit assumptions concerning the limitations of the factor of prestige.

B. A factor of selection. A rather remarkable feature of the procedure is that during the second rating the statements were followed by the names of their *true* authors. The reverse of the experiment was not done; namely, to present statements, originally correctly identified, now joined to false authors asserted to be correct. The investigation centers, in fact, on the effect upon ratings of the transition from objectively false to objectively true authorship![9] While it is not easy to predict what the results would show if the transition were in the opposed direction, two conditions they require present problems of considerable interest. This question is touched upon in the study of Saadi and Farnsworth (5); unfortunately, the irregularity of the results and the failure to inquire into their psychological content render difficult a clear interpretation.

[8] We do not intend to question that the views of noted persons may, under certain social and personal conditions, be effective outside their special sphere of competence. Indeed, the character of such effects and the

[9] This condition constitutes an additional relation of relevance.

consequences do become clear for the present investigation: (1) Those subjects who have initially, perhaps because of better social knowledge and understanding, identified statements most correctly, contribute of necessity less to the study of the shifts following change of identification, while those subjects who possessed poorer social understanding contribute disproportionately to the results. (2) Further, those passages the content of which was sufficiently clear to enable the subjects to identify them correctly, are relatively excluded in the further study of shifts, which are confined primarily to statements that were less clear in this regard.[10] It must, therefore, be concluded that the procedure of this investigation has unwittingly introduced two forms of selection: a selection of subjects with poorer social knowledge, and a selection of statements that could be referred to different sources.[11]

C. The limitation imposed on cognitive factors. For an investigation of this kind it is obviously necessary to assume that the materials read conveyed some meaning to the subjects, that they possessed some knowledge of the authors—in short, that cognitive factors were at work. Indeed, the investigation of Lorge gains in significance from the inclusion of materials of serious content. Yet the rôle of the cognitive factors is not considered, and an examination of them is lacking. Once cognitive factors are included, a number of definite questions must be faced concerning the relation of knowledge and understanding in the subject to the workings of prestige. It then becomes necessary to ask: What did the subject understand of the content of the statement? What did he know and understand of the character of the author? Was he capable of considering the content of the passage in the light of the character of the author? And what may be the bearing of these facts on the interpretation of the findings? Cognitive factors are admitted, but only to the extent of permitting the presupposed factor of prestige to come into operation. In no other way are they allowed to intrude into the theoretical picture.

D. Abstractness in the treatment of results. Since the concrete reactions of the subjects are not directly reported, all we know of their reactions is confined to the quantitative scores and the conclusions derived from their analysis. At no point in the investigation is the specific content of a single statement taken into account. At no point is the reaction of a single subject to a single statement brought into the picture.

The mass statistical handling of the data enforces an extreme abstraction from the concrete psychological reactions. If we wish to understand the rating of a single statement by one subject we would have to consider at least the following factors: (1) the content of the statement for the subject—how he understood it; (2) the subject's knowledge of the author's views; (3) the subject's reason for the false identification; and (4) his reaction to the correct identification. But the present analysis abstracts from these considerations, dealing entirely with quantitative indices of shifts and with the relations between them.

This abstracting from context is inherent in the statistical handling of the results. Each rating is a rating of the particular statement attributed to a particular author. However, in determin-

[10] To be exact, statements correctly identified were again rated, but with the same author attached to them, a condition which is in this investigation peripheral to the principal conclusions concerning the factors responsible for changes in rating.

[11] A comprehensive procedure would also have required the study of statements presented without authorship, or outside of their context.

ing the extent and direction of shifts, the responses by different subjects to different statements attributed to different authors differently rated are combined, the sole criterion of grouping being the distance on a scale between the ratings of two authors.[12] The treatment throws into the same group statements originally rated high and low. The actual psychological situation is very complex, but the distinctions are wiped out in the manner of dealing with the results. That the rating procedure introduces an artificial feature which is probably not inherent in the psychological situation is hinted at in results of the following kind: Six subjects gave identical ratings to Victor Hugo and Huey Long, fifteen subjects rated George Bernard Shaw and James A. Garfield identically, and so on. It may be seriously doubted whether the equating of such ratings is psychologically meaningful.

A statistical procedure of this kind would be in order if the psychological processes in question had already been clearly established, and if the main problem were to determine certain quantitative relations. The latter, in fact, seems to be the view of the approach under examination. The character of the process is not itself questioned; it is assumed to be established.

VI. A REEXAMINATION OF THE RESULTS

We shall now abstract from all the points mentioned and ask what the quantitative results show in terms of the assumptions of the investigation. What do the results demonstrate within their own context, and what is their relation to the conclusions drawn?

A. Correct matchings of statement and author. While the analysis of Lorge centers exclusively on the second step of his investigation, namely, on the modification of evaluations, the issues cannot be confined to the latter alone. Evidently the same factors that are responsible for changes of evaluation must also be at work in the initial evaluation. It would be helpful, therefore, to establish more closely the rôle of the factor of authorship at the initial step.

In this connection we ask the following question: How correctly did the subjects match statements and authors? Calculations based on the data of the original investigation (2, Table II) show that fully 54 per cent of all the matchings were correct.[13] This result does not as yet permit us to relate the fact of correct matching to the individual subjects or statements, a necessary step if we are to draw more definite conclusions concerning the significance of correct matchings. Fortunately, the correct identification scores were calculated by Lorge for each subject; they are here reproduced in Table I with his permission.

TABLE I

INITIAL MATCHING OF STATEMENT AND AUTHOR (AFTER LORGE)

Correct identification score	Frequency
15–19	2
20–24	32
25–29	43
30–34	17
35–39	4
	98

The median value of correct identifications is 27, in full agreement with the proportion of 54 per cent mentioned above. The interpretation of this result is, nevertheless, still uncertain. In view of the fact that random guessing,

[12] Further, the levels from which the shifts proceed are not discriminated in the analysis, a shift of one step to the highest point on the scale being treated as psychologically identical with a shift of one step from the lowest point on the scale.

[13] There was a total of 2685 correct matchings and of 2304 incorrect matchings.

too, would give a distribution centering about 50 per cent correct choices, it is not excluded that the identifications involved no knowledge of the actual relation between the statements and their true authors.

Tô clarify the problem we turned to the content of the passages for a possible clue. A close scrutiny led to the hypothesis that there were distinct groupings of passages, each of which posed somewhat different problems with regard to identification. There seemed to be one category of passages which a reasonably informed person would be likely to attribute to the true author. This was the case when the author's views were well-known, the passage clear, and the relation between the content of the passage and the outlook of the author harmonious. There seemed to be another category of statements which could with equal plausibility be assigned to either of the two names, whether because the authors were not appreciably different in outlook, or because the content of the statement was not definite. Finally, a third category was distinguished which a conventionally informed person would be likely to identify *incorrectly*, because the quotation seemed in better accord with the known character of the false source than with that of the true source.

To test this hypothesis the following informal test was applied: From the set of 50 statements 20 were selected which seemed, on the basis of their content, to belong clearly in one or the other of the categories described.[14] Then the identification scores of these passages, which had been computed by Lorge, were compared. It should be noted that the selection and grouping of the passages was done without prior knowledge of the individual identification scores. The results appear in Table II.

Table II

Initial Matching of Statement and Author (after Lorge)

Clear source: correct matchings %	Ambiguous source: correct matchings %	Misleading source: correct matchings %
86	49	25
72	59	24
78	45	22
75	68	36
76	56	43
69	66	21
	71	
	39	
Average 76%	57%	29%

We see that there are statements which are matched with their proper

[14] Space does not permit the reproduction of all the statements in this sample; we have therefore selected for purposes of illustration one statement in each category. The name in italics is that of the true author, and the figures appearing in parentheses represent the number of subjects choosing each name as the true author of the passage.

Clear source. "This tremendous wealth (of the United States), these gigantic productive forces, are locked away from the masses who could use them. They are the private property of the small parasitic capitalistic class, which locks up the warehouses and closes the factories in order to compel a growing tribute of profit. This paralysis of economy in the interest of profit, at the cost of starvation and degradation to millions, is enforced by the capitalist government with all its police, courts, jails and military."

Earl Browder (86)
Franklin D. Roosevelt (13)

Ambiguous source. "That our system has avoided the establishment and domination of class has a significant proof in the present administration in Washington. Of the twelve men comprising the President, Vice-President and Cabinet, nine have earned their own way in life without economic inheritance and eight of them started with manual labor."

Herbert Hoover (49)
Calvin Coolidge (50)

Misleading source. "Those who hold and those who are without property have ever formed two distinct classes."

Karl Marx (75)
John Adams (24)

authors with a fairly high degree of correctness, others with a proportion approximating that to be expected by chance, and still others are matched incorrectly with a frequency far exceeding chance. Secondly, our predictions are on the whole in the correct direction, the averages obtained for the three categories selected solely on the basis of their content differing characteristically and in the expected direction. The conclusion that a factor of social information and understanding affected the matchings is trite, but not without interest.[15] At least in the instances in which the matching is not blind we cannot exclude the possibility that it is the utterance or the action that determines the prestige of its author. If such cases are realized, the resulting conception of prestige would be radically different from that of the current approach.

B. Incorrect matching of statement and author. Of particular consequence for the present examination are the incorrect matchings on which the main conclusions depend. Since the initial ratings of statements are treated in terms of their dependence on the prestige of the matched authors, it seems necessary to inquire into the character of this relation.

A first survey of the data reveals an enormous scatter of responses (see 2, Table II, as well as Table III below). In the initial step of the investigation we find high-ranking statements assigned to authors of low rank, and vice versa. Similarly with regard to shifts: Large changes in prestige of authors are often followed by little or no change in the rating of statements, and in a fairly large number of instances the changes are in a direction opposed to the shift in prestige of authors. One has the impression of many factors, working in different and unknown ways.

The situation may be schematically represented below, with the arrows showing the direction of change, and the dash representing no change:

Differences in author-ratings	Shifts of statement-ratings		
	a	b	c
1) ↗	↗	↙	—
2) ↙	↗	↙	—
3) —	↗	↙	—

While of the nine possible pairings of directions only three (1a, 2b, and 3c) are consistent with the assumed theory, all other variations are realized, including 1b and 2a, which are in direct contradiction to it. To be sure, these results in no way deny the factual conclusions of Lorge, nor do they necessarily minimize their possible significance, but they do also point to the presence of a factor or factors other than prestige, the character of which remains unknown. It seems necessary to conclude that the evaluation of the statements is to a substantial degree determined by considerations other than their authorship, correct or imputed.

In order to survey more clearly the relations between the incorrect matchings of statements and authors we have regrouped the data of Lorge (2, Table II) in Table III below.

We observe in Table III, consistently with the description of Lorge, a trend for the ratings of author and statement to be related. But this is, as mentioned earlier, by no means the sole trend. This becomes especially clear if we compare the total distributions of ratings of authors and statements.

[15] Correct matchings in this investigation probably depended only to a negligible extent on specific recall. In the great majority of instances such recall was simply not possible. The extent of correct identification may be taken as a sign of the understanding of certain social issues and of their relation to the outlook of certain public figures.

Table III

Initial Ratings of Statements and (False) Authors (Based on Table II of Lorge)[16]

Ratings of statements	Ratings of false authors					
	F1	F2	F3	F4	F5	Σ
1	243	227	177	62	57	766
2	72	132	120	52	55	431
3	32	70	159	64	101	426
4	24	62	87	44	73	290
5	26	35	89	56	185	391
	397	526	632	278	471	2304

There is evident one notable disparity between the distributions which will turn out subsequently to be significant for an understanding of one of the conclusions, that relating to the law of effect (see p. 271). There is a strong concentration of '1' ratings of statements which is not paralleled by a similar concentration in the author-ratings, the former occurring with twice the frequency of the latter. A larger proportion of statements than of authors is highly rated. It is necessary to conclude that the assumption, required by the investigation, that the ratings of the statements are a function of the ratings of the authors is subject to the restrictions evident in Table III.

3. *The strength of the 'prestige' effect.* It is obviously important to determine the strength of the effect introduced by the factor of authorship. The marked scatter of the results described earlier suggests that the effect is limited. That this is in fact the case becomes evident in an examination of Lorge's results. In Table IV we reproduce the relevant data.

The changes in the ratings of the statements correspond to the differences in the ratings of the authors. At the same time, however, we observe that the magnitude of statement-shifts is invariably less, and often considerably so, than the corresponding magnitudes in author-differences. For example, the maximum shift of the statements (in terms of mean measurements) does not appreciably exceed one step on the scale, while the corresponding prestige differences vary to the full extent of the scale.

It would be important to see more concretely at what points the experimental factor fails to be effective. This we cannot do from the results of Table IV, which contain only average determinations. For this purpose it was decided to return to the original data and to apply a fractionating procedure now to be described. The first step was to select that category of statements whose authors had received *identical* ratings during the transition from wrong to

[16] The figures in the table are to be read as follows: Out of a total of 766 statements receiving a rating of 1, 243 were assigned to false authors rated 1, 227 were attributed to false authors rated 2, and so on. Similarly, out of a total of 397 statements matched to authors rated 1, 243 received a rating of 1, 72 received a rating of 2, and so on.

Table IV

Comparison of Shifts in Ratings of Statements and Differences in Ratings of Authors (Based on Table IV of Lorge)

1. Differences in ratings of authors	−4	−3	−2	−1	0	1	2	3	4
2. Mean shifts in ratings of statements	−.190	−.300	−.172	−.131	.034	.314	.557	.935	.960
3. Median shifts in ratings of statements	−.160	−.420	−.235	−.250	.041	.388	.766	1.390	1.370

true identification, and to calculate the corresponding amount of shift in the ratings of the statements. This category—which was, fortunately for the present purpose, quite large, containing 583 cases—has the following important property: Any changes in the ratings of a statement obtained under these circumstances cannot be due to the effects of prestige as here defined, being rather a measure of the extent of change when the factor of prestige is strictly excluded. The importance of this measure consists in providing a control point or a basal level from which the size of changes that may be due to prestige can be calculated. The results appear in Table V.

We find: One-half of the statements in the control category undergo no change in ratings; the others show changes, the frequency of which decreases with the magnitude of change. There is also a slight tendency for changes in the positive direction to predominate. These results describe the behavior of shifts in statements when

TABLE V

AMOUNT AND DIRECTION OF STATEMENT-SHIFTS

Changes in ratings of statements with:						
	Zero shift in author-rating		Positive shift in author-rating		Negative shift in author-rating	
Statement-shifts	*N*	%	*N*	%	*N*	%
−4	4	.01	2	.00	19	.03
−3	15	.03	10	.01	23	.03
−2	30	.05	31	.03	61	.08
−1	77	.13	82	.09	140	.18
0	301	.52	364	.41	408	.51
1	102	.17	173	.19	95	.12
2	40	.07	137	.16	36	.05
3	10	.02	61	.07	9	.01
4	4	.01	30	.03	8	.01
	583		890		799	

the factor of prestige is excluded. It is now possible to compare with these data the shifts taking place when the factor of prestige, as understood in the original investigation, is at work.

We further fractionated the data by calculating the distribution of shifts in all responses (a) with an upward change of author-ratings, and (b) with a downward change in author-ratings. These distributions are also reported in Table V. In entire agreement with the report of Lorge, we find a predominance of positive changes with an upward shift of the authors, and a predominance of negative changes in response to a downward shift of the authors. Equally in agreement with Lorge is the fact that the positive changes in the former case are more pronounced than the negative changes in the latter case.[17]

At the same time, we note certain other features of the results which are seriously at variance with those just described. In each of the last two distributions there occur changes which are opposed in direction to the changes of authorship. This is, of course, in contradiction to the assumed theory. Most noteworthy, however, because most unexpected, is the striking closeness of the three distributions in the following regard: All distributions show a great concentration at the point of zero change! This is a remarkable result, for which neither the theory nor the factual conclusions of the original investigation have prepared us.

These final results show, on the whole, small differences when compared with the control data. Whatever else we may say concerning these results, it is clear that the introduction of the experimental factor had a moderate effect. On the basis of the findings one would

[17] It would be of interest in this connection also to determine the shifts in ratings of those statements which were initially correctly identified.

be required to conclude that the effect studied was *weak*. Further, on the basis of the remarkable magnitude of the zero shifts it would be necessary to conclude, in terms of this approach, that there was a trend *not* to shift in one's evaluation of statements, which was at least as strong as the tendency to change evaluations. This trend surely requires explanation, especially since it may have a bearing on the assumed character of prestige.[18]

That the effect obtained is limited does not of course cast doubt on its presence. It is, however, noteworthy that the psychological characterization of the factor of prestige does not include a reference to limitations in its operation.

Finally, the findings give rise to certain empirical questions. Were there statements which responded readily to the factor of authorship, and were other statements more resistant to change of evaluation? If so, what was the character of each? Similarly, we might inquire whether the subjects responded in different ways to the factor of authorship. To some of these questions we turn in the following section.

4. *A further fractionating of the results: Analysis of statement*. It would be of particular interest to establish whether there were statements that differed in responsiveness to the factor of authorship, and to compare them with respect to their content. Fortunately Lorge had computed the correlation for each statement between the shifts in its ratings and the corresponding differences in author-ratings. These correlations, which were not published in the condensed original report, were made available to the writer. From the total of 50 statements we selected 3 showing the highest correlations and 3 whose correlations bordered on zero; these are reproduced below. The selection is admittedly not comprehensive, but the procedure may be illustrative of the direction investigation should follow.

Statements shifting considerably in ratings

Statement 12 ($r = .66$). "Plenty is at our doorstep, but a generous use of it languishes in the very sight of supply.

"Primarily this is because the rulers of the exchange of mankind's goods have failed through their own stubbornness and their own incompetence, have admitted their failure and have abdicated. Practices of the unscrupulous money changers stand indicted in the court of public opinion, rejected by the hearts and minds of men.

"True, they have tried, but their efforts have been cast in the pattern of an outworn tradition. Forced by failure of credit, they have proposed only the lending of more money. Stripped of the lure of profit by which to induce our people to follow their false leadership, they have resorted to exhortations, pleading tearfully that confidence be restored. They know only the rules of a generation of self-seekers."

Earl Browder
Franklin D. Roosevelt

Statement 29 ($r = .71$). "Of course I must not be supposed to imply that the means to this end (the revolution) will be everywhere the same. We know that special regard must be paid to the institutions, customs and traditions of various lands, and we do not deny that there are certain countries such as the United States and England, in which the workers may hope to secure their ends by peaceful means."

Karl Marx
William Jennings Bryan

Statement 31 ($r = .50$). "Capital cares nothing for the length of life of labor-power. All that concerns it is simply and solely the maximum of labor-power, that can be rendered fluent in a working day. It attains this end by shortening the extent of the laborer's life as a greedy

[18] The magnitude of zero shifts requires, of course, to be explained also if one assumes the alternative interpretation described earlier.

farmer snatches increased produce from the soil by robbing it of its fertility."

Daniel Webster
Karl Marx

Statements not shifting in ratings

Statement 2 ($r = .08$). "It is of no consequence by what name you call the people, whether by that of freemen or slaves; in some countries the laboring poor are called freemen, in others they are called slaves; but the difference as to the state is imaginary only. What matters is whether a landlord employing ten laborers on his farm gives them annually as much money as will buy them the necessaries of life or gives them those necessaries at short hand."

James Madison
Robert Owen

Statement 11 ($r = .05$). "Service is the foundation of successful business. . . . The only difficulty that anyone will meet through absolute devotion to service in business has to do with profits. These will be embarrassingly large."

Henry Ford
William Randolph Hearst

Statement 13 ($r = .01$). "This tremendous wealth (of the United States), those gigantic productive forces, are locked away from the masses who could use them. They are the private property of the small parasitic capitalistic class, which locks up the warehouses and closes the factories in order to compel a growing tribute of profit. This analysis of economy in the interest of profit, at the cost of starvation and degradation to millions, is enforced by the capitalist government with all its police, courts, jails and military."

Earl Browder
Franklin D. Roosevelt

Though a final conclusion would have to be based on the concrete reports of the subjects and on a more comprehensive group of statements, the suggestion is warranted on the basis of the present examination that there were consistent differences between the two groups of statements. The statements in the second category were generally so definite that a change of authorship could not seriously alter their content; indeed, they would be more likely to alter one's view of those whose utterances they were. While the statements of the first category seem to be more heterogeneous, a number do lend themselves to different interpretations according to their authorship.

The present analysis shows that the conclusions do not apply to the entire set of statements. It could also be shown by a similar analysis that the conclusions do not hold for the entire group of subjects.

5. *The interpretation in terms of the law of effect.* The results of the original investigation reveal a consistent difference between the reactions to positive and negative changes in authorship. Positive shifts to a positive change in authorship are greater than negative shifts to a negative change in authorship (see 2, Table IV and Table V of the present report). This finding is interpreted in terms of the law of effect. It is assumed that a favored author functions psychologically in a way analogous to the 'confirming reaction' of a reward, while one of low esteem functions as a 'punishment.' Since the consequences of rewards are said to be stronger than those of punishments, the findings are subsumed under these categories. "The results of the present experiment may be considered as contributing further data to the field of reward and punishment. They may be interpreted in the light of the known facts about rewards and punishments" (2, p. 401).

It is not excluded that future investigation in the dynamics of preferences will bring forward results that bear an external resemblance to the one under consideration. There is for example the likelihood that after one has seen the positive sense of an utterance or action there develops a resistance to seeing it in a poorer light. To go away from the good sense of a thing may be more difficult for many than to go away from a poor sense; or there may be a tendency to jump to the better sense, relieved to see the improvement. Processes of this order may indeed be related to basic characteristics of persons, and may prove to be of importance in future research. We may find, for example, that there are persons labile in both directions—in seeing the better side and in becoming suspicious; some may be labile mainly in the direction of suspicion, while still others may be glad to abandon suspicion and to assume quickly a trusting attitude. It is evident, however, that the law of effect is not oriented to such processes. Its application in the present investigation has a far different purpose, namely, to establish a conception of 'sheer' prestige, acting irrespective of reasonable considerations. If, on the other hand, intrinsic characteristics are decisive, then negative factors should, under certain conditions, be as effective or more so than positive factors. It is therefore of some importance to examine the results more closely.

It is first necessary to clarify the possible relation of the law of effect to the present investigation. Strictly considered, it can account only for the *repetition* of a response on a subsequent occasion. It cannot account for the direction of change in responses, which is presumably the center of the investigation. Within these limitations it would be consistent with the law of effect if positive factors induced greater changes in some direction or other than negative factors.

In studying the data it seemed most simple to determine whether the results with regard to positive and negative changes were based on data which were equivalent. We therefore turned to an examination and comparison of the *initial* ratings of those statements which were later to shift in positive and negative directions, respectively. Our aim was to establish whether there was any clear difference between the evaluation of these statements at the start of the investigation. The calculations, based on Tables II and IV of Lorge, are re-

TABLE VI

DISTRIBUTION OF INITIAL RATINGS OF STATEMENTS (IN %)
(BASED ON TABLES II AND IV OF LORGE)

Initial ratings of statements	Differences in author-ratings							
	−4	−3	−2	−1	1	2	3	4
1	.70	.48	.51	.41	.30	.23	.14	.18
2	.09	.16	.20	.22	.18	.18	.12	.19
3	.02	.14	.11	.15	.20	.23	.25	.26
4	.09	.13	.08	.09	.16	.17	.21	.14
5	.10	.09	.09	.12	.17	.18	.28	.23
Mean shifts in statement-ratings	−.190	−.300	−.172	−.131	.314	.557	.935	.960

The table is to be read as follows. Of the statements whose author-differences amounted to −4 points, 70 per cent were initially rated '1,' 9 per cent were rated '2,' and so on. Similarly, of the statements whose author-differences amounted to 4 points, 18 per cent were rated '1,' 19 per cent were rated 2, and so on. The figures of the last row describe the shift in statement corresponding to each category.

ported in Table VI. They include the initial ratings of statements categorized on the basis of the direction and amount of shift in prestige of the author.

Grouping the data in this manner, the following striking trend became evident: Those statements which were later to change in a negative direction contained an unusually large proportion of high ('1') initial ratings, while those statements which subsequently shifted in the positive direction simply did not contain a correspondingly large proportion of low ('5') initial ratings. This becomes even clearer when we examine the summary results of the two categories of statements which will be found in Table VII.

TABLE VII

DISTRIBUTION OF INITIAL RATINGS OF STATEMENTS

Initial ratings of statements	Positive differences in author-ratings		Negative differences in author-ratings	
	N	%	N	%
1	217	23	363	47
2	160	17	158	20
3	214	23	101	13
4	155	17	70	9
5	187	20	84	11

We see that the two categories differed considerably with regard to their initial ratings; the one was recruited predominantly from statements rated highly, while in the other category the statements were more equally distributed over the entire scale. It is now evident that the large proportion of high ratings, to which we called attention in Table II, had a serious bearing upon the present results.

The facts just described remove all ground for the factual conclusion about a difference in the strength of positive and negative changes, and render superfluous the appeal to the law of effect. The results which led Lorge to these conclusions were produced by an accidental difference (accidental to the issue of the law of effect) in the distribution of initial ratings. A psychologically simpler explanation would be that statements which are so clear-cut to a subject that he rates them at the extremes of the rating scale are less likely to be shifted with a change in authorship than more ambiguous statements. It would follow from this assumption that the changes would be weaker in any category containing extreme statements.

VII. SHERIF'S INVESTIGATION OF PRESTIGE

A careful examination would disclose that the main conclusions we have reached in the preceding sections apply generally to investigations in this region. They would be found to share both certain features of procedure and certain characteristics in their results. The neglect of cognitive processes in evaluation, the ignoring of contradictory results, the weakness of the effects attributed to prestige or suggestion would be found generally. The findings reported often seem to be in accordance with Thesis II. While space forbids their separate discussion it may be helpful to submit to scrutiny one further investigation that differs in an interesting regard from that of Lorge. We have selected Sherif's investigation of prestige (6) principally because an interpretation of it poses certain new problems not hitherto considered.

Sherif undertook to demonstrate that the factor of prestige affects the evaluation of literary materials. In essential regards the procedure is identical with that of Lorge. Groups of college students ranked a set of 16 brief prose passages, each consisting of three or four lines, according to their literary quality. Each passage came with the name of a well-known author. Previously the same groups had ranked these authors in terms of their literary standing.

One feature of the experiment was hidden from the subjects: The prose passages were all selected from the writings of one author (Robert Louis Stevenson). Further, they were selected so as not to differ appreciably in quality. There is, in short, no relation between the passages and the authors to whom they are imputed.

Yet there is a positive relation between the ranking by subjects of the passages and their previous ranking of the authors to whom the passages are now attributed; passages identified with highly regarded authors receive higher rankings. This finding is confirmed in further experiments with new groups; this time the identical passages are coupled with the identical authors in new pairings. The result is that the same passage tends to be differently ranked, depending on the author's name. There was also a minority of subjects who deliberately disregarded the authors, and whose results were therefore negative.

As in the preceding investigation we note that the results, while positive, are also moderate; the average correlations reported by Sherif range from .30 to .53. Clearly there are factors other than prestige at work, and further, these are far stronger than the prestige factor.

On this basis the conclusion is drawn that a factor of prestige-suggestion determined the changing literary evaluations. The positive results are taken as confirmation of the assumption that evaluations have been altered by the introduction of prestige. The interpretation is offered that "Authors rated high tended to push up the ratings of passages attributed to them. Conversely, authors rated low tended to pull down the ratings of passages attributed to them. . . . *Not the intrinsic merits of the passages* but the familiar or unfamiliar frame of reference explained the findings" (6, p. 122. Italics are those of the present author).

It is by no means unlikely that the evaluation of a literary work depends often on one's understanding of the times in which it was produced, on the stage of development of its author, and on related factors. But it is equally clear that evaluations of this order were excluded from the scope of the investigation. Does it follow that arbitrary operation of prestige was at work? More concretely, have we ground for concluding that the results of Sherif represent an actual, even if episodic, change of evaluation?

To answer these questions a more complete knowledge of the ways in which the subjects dealt with the situation would be necessary. While it is unsatisfactory to reconstruct such processes for subjects who cannot speak for themselves, we shall attempt to do so in the hope of clarifying a problem. This will permit us to propose an alternative interpretation which, while tentative, may be deserving of experimental check.

First we note the fact that the subjects are faced with an insoluble task. They are asked to find differences between materials which were carefully selected so as not to differ appreciably. In short, the possibility of an objective solution has been excluded by the conditions of the experiment. (To be sure, the procedure is entirely justified, but it makes all the more necessary the task of ascertaining how the subject copes with it.) Faced by this situation the subjects have the possibility of rejecting the task on the ground that it cannot be fulfilled. In that case they would have acted as 'critical' persons, resistant to the spurious factor of prestige. That they do not do so, that they nevertheless continue with the task, is the fact of importance that needs explanation. According to Sherif they proceed to alter their evaluations. Do they?

We shall assume that the failure of the subjects to reject the task is in the first instance due to the social relations prevailing between them and the experimenter, as well as between each other, and that these constrain them to take the task seriously. The social setting may indeed be of decisive importance. Were the same experiment to be performed between intimate friends, it is quite conceivable that the outcome would be entirely different and indeed that the experiment could not be carried out. That the group trusts the experimenter and seeks to find in the material what the investigator has presumably been able to discern—this seems to be a particularly important feature of the given conditions. There is possibly another social factor also at work, no less important. Each subject is aware of his own uncertainty, but not of the qualms of his neighbors, who appear to him to be simply attending to the task. In addition, therefore, to the strong force between him and the mature experimenter, there is now provoked the desire not to appear ridiculous before others. The effect of these factors is to bring the subject within the orbit of the situation.

The subject feels himself under the necessity of arriving at a judgment for which he has no reasonable basis. He may be embarrassed to find that he is unable to do what the experimenter and the group appear to regard as obvious. He then proceeds to clutch at whatever clues he can find. It so happens that the experimenter has obligingly placed some in the way. Casting for a way out, the subject makes use of them—though to a surprisingly moderate extent. But, at the same time, the forces that prevent him from leaving the task also alter its character for him. No longer is he concerned with the question: "What differences can I observe between these materials?" Now the task becomes: "Which of these am I *expected* to like and dislike?" With this transformation the subject is *no longer evaluating*. His reluctant 'responses' may carry no conviction even to himself, nor might he care in the slightest to defend them.

The alternative interpretation we have here sketched is admittedly incomplete; it would almost certainly need revision in the light of concrete investigation; it might turn out to be incorrect. In particular, it slights the rich individual differences that would come to light in concrete investigation. But it has the merit of providing an alternative and, therefore, of sharpening our procedure and observation. It is in this connection relevant to mention an infrequent but psychologically highly interesting reaction described by Sherif. He notes the case of a subject who "suspected that the names under the passages were not those of the real authors. Consequently, in her case, the magic of the author's name did not work. She took a cautious, or one may say, a negative stand, arranging the passages in her own way" (6, p. 123). It is of interest that there were at least some who did not wish to be affected by external factors and took the fairly intelligent step of hiding the authors' names from themselves. But so entrenched is the assumption about the blind character of the process that when a subject attempts to avoid the experimental trap, the conclusion is drawn that he is taking a negative stand. There seems to be little hope for the human culprit when he faces the psychological bar. He has only the choice of being impaled on the positive or negative horn of irrationality.

The almost complete absence of protest on the part of the subjects is perhaps the feature of most significance in the investigation, and one that merits investigation. Nevertheless, if one were to assert that the submission to the social forces in the situation *is* the factor of prestige, he would be not only shifting but also confusing the issue. To apply the term prestige to the social forces in question would be no less futile than is the application of the term to the problem of evaluation; the social forces, too, need to be described psychologically before they are labeled. Examination might reveal that these contain some processes wholly different from the assumed character of prestige. That the subjects take the experimenter and the group seriously, that they start with an attitude of trust rather than suspicion, has good reasons: It would have been far more blind systematically to disbelieve from the outset those with whom one stands in a cooperative relation. Even the fact that they continued to bend to the pressure to the end is an understandable and appropriate reaction in some of the subjects—if we may assume that they were preoccupied with more pressing matters and simply wished to be rid of the task. Where some may have failed both themselves and each other was in their lack of firmness in acting, in their indifference to being placed in a questionable situation, in their failure to stand up to the discomforts of conspicuousness and ridicule. Were we to examine the reasons for this apathy, then we might have to take into account not the feeble and perhaps spurious factor of prestige, but the rôle of very 'practical' factors of narrow self-interest, directed by strong social forces. Then we might even discover that the process of education itself often supports this attitude, and that a not inconsiderable rôle in this process is to be credited to a certain concept of prestige.

VIII. Conclusion and Summary

We have examined an experimental procedure in social psychology that has served as the basis for far-reaching conclusions concerning the processes of evaluation. The results have appeared to confirm the proposition that social judgment and action are arbitrarily established, and that they can be manipulated at will. It was further concluded, at least implicitly, that this formulation is generally valid for social processes. The formation and change of political, aesthetic, and moral evaluations have been accordingly treated almost exclusively as an expression of blind bias. Changes of evaluation became synonymous with the manipulation of evaluations and the formation of beliefs and attitudes was equated to a process of 'uncritical acceptance' of beliefs and attitudes.

While we have touched only incidentally on the validity of these propositions, the evidence is nevertheless sufficient to raise a doubt and to prompt a question. Where, we may ask, is the arbitrariness, the bias, the blindness of which the current experimental approach is so deeply enamoured? Does it exist in the minds and actions of the subjects, or is it to be found in the mode of investigation itself and in the psychological conceptions that inspire it? [19]

We must, however, return to our main problem, namely, to the technical character of the current approach. Here we find that the interpretations often are at variance with or do not follow rigorously from the findings. Negative or even contradictory instances tend to be considered in a purely statistical way,

[19] It may be necessary to point out that we do not question the occurrence of 'uncritical' processes. We doubt, however, whether the approach described is capable of dealing with those any more adequately than with reasonable processes.

not being allowed to intrude on the initial assumptions. Effects are at times disregarded which turn out upon examination to be quite strong, while the effect which is regarded as central is found to be quite weak. But most important, the presence of the sought-for effect is unquestioningly taken as confirmation of the initial assumptions.

The difficulty seems to reside quite clearly in the failure to submit the theoretical assumptions to direct investigation. Because no process other than that assumed is admitted as a possibility, the technical steps are unwittingly suited to its dimensions, with the consequence that procedures and interpretations are throughout controlled by the starting point. The aim of investigation is now reduced solely to demonstrating an assumption in quantitative terms. There is then the danger that if the theory is simply not appropriate, the results, moving in a tangent direction, may still appear to confirm it for secondary reasons, especially if one is content with exclusively statistical proofs.

What is perhaps most difficult to see is that the approach suffers from an inherent lack of rigor, both theoretical and experimental, despite its seeming outward exactitude. Instead of providing a necessary aid to investigation, quantitative procedures are applied in a rigid manner with the consequence that they black out the perception of problems and the facing of alternatives. It seems to be assumed, at least in some cases, that there is an opposition between quantitative exactness and the examination of psychological processes. It is only in this way that we are able to explain the neglect of the concrete basis of evaluations.

It may seem that we have been unduly critical of an approach that does, in all fairness, employ a procedure which permits the results to answer for or against a given assumption, and that we are in effect demanding of investigation that it start in advance with hypotheses that will surely turn out to be relevant. Even if we may not insist upon the latter condition, it is not too much to ask that an experimental procedure be able to revise its assumptions in the light of its own findings. More is required of experimentation in social psychology at the present time than to follow unquestioningly ideas that were formulated quite independently of investigation. When, as is the case well-nigh universally in social psychology today, the character of the most simple processes still remains to be formulated, procedures of the kind here described are inadequate and may be misleading.

To summarize:

1. We have described a widely used approach to the investigation of group influences and examined its presuppositions.

2. Two interpretations of the character of group forces were confronted. It is the central assumption of the current experimental approach (Thesis I) that the evaluation of issues can be manipulated in indifference to their content or merit. The alternative interpretation we proposed (Thesis II) asserts that changes of evaluation require the transformation of content in response to altered contexts.

3. The following major differences between the two interpretations were described: (a) Thesis I asserts that a change of evaluation consists of a change of response to a constant stimulus, the primary process being the change of a stimulus-response connection, or of the strength of a given connection. According to Thesis II the character of the stimulus-situation changes in accordance with its new rôle in a changed framework. (b) Thesis I assumes a process strictly different from

the grasping of the character of a given situation, different from discovering relations of fitness or contradiction. It excludes the rôle of understanding, except insofar as the latter furthers the operation of the essentially unreasonable factor of prestige. For Thesis II the main factor at work in this region of investigation is a change in the cognitive character of the given situation. (c) In terms of Thesis I the essential factors responsible for social evaluation are emotional forces divorced from and overriding questions of actual merit. Thesis II asserts that emotional processes are as a rule under the direction of cognitive factors, and are controlled by the trend to find relevant relations.

4. The experimental procedure in terms of the first approach contains a number of characteristic shortcomings: (a) It fails to investigate directly the processes under investigation, the latter being indirectly inferred from quantitative indices. In consequence treatment of the results abstracts to an extreme degree from the concrete psychological reaction. (b) It systematically neglects the rôle of cognitive factors. (c) It retains fundamentally reasonable relations which are not required by Thesis I, and which should have been eliminated in a strict test of it.

5. The findings contain certain facts not predictable from and at variance with the assumptions of the first approach: (a) The effects interpreted as due to suggestion or prestige are not general and are often weak. The theoretical formulation fails to account for the limitation of the effects. (b) In a substantial number of instances the effects obtained contradict the theoretical assumptions.

6. The conclusion was drawn that the investigations examined have not dealt with the process they were presumably studying—that of blind suggestion or prestige—and that they did deal, though unknowingly, with processes of social understanding. It was proposed that an adaptation of the current procedure might throw light on processes of comprehension of social issues.

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THE SIGN OF A SYMBOL: A REPLY TO PROFESSOR ALLPORT

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"I should take more interest in animals if I were less interested in men," Robert had said. And Vincent had replied: ". . . There is more to be learnt, if one can use one's eyes, in a poultry-yard, or a kennel, . . . than in all your books, or even, believe me, in the society of men, where everything is more or less sophisticated."—André Gide, *The Counterfeiters.*

I. Animals and Humans

In a recent article Professor Allport (1) sounded a note that has become increasingly insistent of late (43, 61). Speaking as president of the Division of Personality and Social Psychology of the American Psychological Association, he voiced his concern over the tendency of theorists to build their conceptions of human nature on mechanical models and on studies of animal and infant behavior. These sources, he contended, are retarding psychology in its attempt to meet the enormous responsibilities facing it in the world today.

There is no doubt that Allport has touched many of his colleagues, including the writer, on a sensitive spot. Few will deny the compelling need for trained investigators to tackle the immediate problems of a society struggling to survive. Indeed, if or when the present crisis passes there will always be such a need. If Allport had been content to base his protest solely on the ground of urgency, there might be nothing more to say. But he made it quite clear that he means more than that. The models he disapproves of are not only too slow, they are intrinsically defective. It is not enough to postpone their use 'for the duration.' "Must we now resume the *tattered stencils* that we so recently abandoned with such good effect?" he asks (p. 191; italics mine). Again, even if the world could wait a thousand years, says Allport, "I question whether we should endorse this counsel of patience or the premises upon which it rests" (p. 183). It is evident, then, that the issue is one of more than temporary significance and thus becomes more than a personal problem.

Let us define the issue clearly. It is necessary to do so because there is another, more basic issue which might otherwise be confused with the one at hand. That other issue is the old question of whether the methods developed in the natural sciences can be fruitfully applied to human morals. One danger of Allport's position is that it appeals to the impatient who are ready to abandon, along with the 'outworn models' he decries, the whole methodology that has grown up around them, the rigorous definition of constructs, the careful coordination of logical and experimental procedures. The point at issue, however, is not the status of psychology as a science. It is simply whether psychologists are directing their scientific tools toward the proper subject matter.

Of the three models in question—machine, animal, and child—I shall confine myself to the animal, the one, as it happens, against which Allport fired most of his ammunition. We may start with the assumption that the student of animal behavior must eventually justify his efforts by the contribution he can make to *human* psychology. This is the position taken by Tolman in his disarming defense of the laboratory rat (71). His thesis, elaborated in the three areas of intelligence, motivation, and emotional stability, is that the formal laws governing behavior can be sought as well in rats as in humans—and more conveniently. Tolman admits, even insists, that the particular abilities, goals, and conflicts of men are largely cultural products and must be studied where they are found. But he rests his case on the premise that *the basic behavioral mechanisms of rats and men are homologous.* Here, then, we come to the heart of the matter. Here is the premise which Allport questions and which we are called upon to examine.

Wherein do animals fall short of the requirements of human psychology? In at least two respects, Allport contends. The first is their limited motivational structure; the second, more basic in that it partly underlies the first, is their inability to deal with their environment in symbolic terms. Let us deal with these problems in order.

II. Intentions and Expectations

The difference between human and animal motivation, as Allport so neatly phrases it, is that humans intend, while animals merely expect. Intention he defines as *"what the individual is trying to do"* (p. 186), and goes on to describe it as usually, but not necessarily, conscious and as directed toward the future. As Allport is well aware, such a definition makes hardly a gesture toward operational rigor, and the supplementary remarks do not help much. As to the latter, we may note in passing that if an intention *is* unconscious, in the sense of not reportable, its presence must be inferred from behavior, just as it may be so inferred even if conscious. It is thus not essentially different from other 'intervening variables'—drive, habit strength, demand, hypothesis—that, properly defined, play a useful rôle in behavior theory. The further statement that an intention is directed toward the future may be highly important to the social practitioner and to the metaphysician, but it is altogether irrelevant to the scientist seeking to explain the intention.

It is interesting, and may be significant, that the most notable attempts to provide objective criteria for intentions have been made by 'animal' psychologists. Tolman has made as free use of animal models as any other theorist. Yet far from excluding purpose, Tolman makes it, with cognition, the distinguishing feature of behavior (69). Its identifying marks are two: (1) persistence in getting to a goal-object, and (2) docility in choosing shorter routes to get there. But what of expectancy? Allport conceives of this process as largely passive, yet at the same time the closest approach to intention in animals below man. Tolman himself always used the construct as one of a number of intervening variables, never the sole determiner of behavior. Recently he and his coworkers have defined it by means of a set of specific experimental conditions (74, p. 15). It is noteworthy that their definition starts with deprivation of food and ends with a definite act (*i.e.*, running down a path in a certain direction). As so defined, expectancy is hardly passive.

But Allport is not satisfied with these attempts. On the surface he appears to object to a tendency of animal psychologists to deal separately with needs and knowledge and a failure ever to get them together. Such a criticism is easily refuted, partly on the ground of convenience in treatment, partly by referring to definitions such as the one just mentioned or Hull's definition of reaction potential as a product of primary drive times habit strength (33, p. 253).

But Allport's objection, if I understand his position, goes deeper. He believes that because animals are incapable of the intricacies of thinking and feeling that enter into human motives, animal behaviorists will be forever unequal to the task of understanding them. He may be right, but so far there has appeared no easily available shortcut to that goal. If the goal is to be attained at all it will be by the careful formulation of experimental conditions and resultant behavior. Animal psychologists have taken the lead along this path.

Allport is equally dissatisfied with Hull's concept of the fractional antedating goal reaction as an equivalent of intention. His reasons for rejecting it throw further light on his position. Hull's construct is too immediate for the "long-range orientation which is the essence of morality" (p. 188), too specific for the vague 'directive schemata' that constitute human values.

The first point to be made in this connection is that remoteness is not different in kind from immediacy, nor is generality to be sharply distinguished from specificity. Each pair of terms stands for degrees of a common property and can be represented by points on a continuum. But if we admit the continuity of human and animal motives in these respects, there is no obvious reason why their formal relations should not be studied at one point as well as at another. In the second place, these very properties of motives, their temporal range and their 'precision,' are variables determined by circumstances and in turn affecting behavior. As such they deserve to be investigated in their own right, and again we find that on the whole the most promising relevant studies have been made with animals.

Little has been done so far to define the conditions under which proximate as compared with ultimate goals are established. But the effects of temporal remoteness of goal on performance have been intensively studied. Here belongs the work of Hamilton (25), Roberts (60), and Wolfe (80) on delay of reward, and of Grice (23), Perin (54, 55), and Perkins (56) on the gradient of reinforcement, to mention only a few. Here, too, belongs Mowrer and Ullman's analysis of the role of temporal factors in conflict resolution (52), a study in which, incidentally, the authors make good use of an experiment with rats to illustrate the dynamics of a vital problem in human adjustment.

Specificity of goal is another property the variation of which can be, and has been, profitably studied at the animal level. I refer to Young's work on food preferences as determined by bodily needs (*e.g.*, 85, 86), to Beach's studies on the adequacy of various sex objects as a function of hormone injections and of experience (2), to the demonstrations by Tinklepaugh (68), Elliott (17), and Crespi (12) that the effectiveness of a reward depends on the kind or amount 'expected.' But our interest in human motives includes not only ends, or ultimate goals, but means, or subgoals. 'Non-specific schemata' may sometimes refer to the possible techniques for attaining an objective—money or prestige, for example—rather than the objective itself. Such undefined regions, to adopt Lewin's term, usually become better structured as a result of attempts to reach the goal, and we should expect them to as the adolescent gropes toward maturity. But is this process found only among humans? Something of the same sort seems to happen when a rat learns a maze. According to Lewin (46), maze learning consists essentially of an increasing specificity of subgoals. And as Dashiell (14), Dennis (15), and Witkin (79) have shown, under certain conditions even a rat's subgoals do not become altogether specific.

To illustrate the supposed distinction between the animal governed by habit and the human guided by rather foggy intentions, Allport uses Lecky's hypothetical case of the thumb-sucking child who spontaneously abandons the practice when he gets a new concept of himself. True, we can hardly claim a self-regarding sentiment for rats. But we can find instances in which rats abruptly dropped a well fixed habit when a change of conditions was introduced. Honzik and Tolman (30), for example, trained rats to choose the shorter of two elevated paths to concealed endboxes containing food. When the habit was firmly entrenched, the rats were shocked in the preferred endbox, whereupon most of them reversed their subsequent choices. Control tests showed that reversal depended on visual perception of spatial relations, a process which suggests, however remotely, a 'directive schema.' Of course, it may be argued that the element of spontaneity was lacking, but the same objection may be raised in connection with the thumb-sucker bombarded by social pressures from young and old. There is no need to stretch the analogy to make the point that human and animal motives do not function so differently after all.

III. Signals and Symbols

The second respect in which Allport finds the animal model wanting is its failure to use symbols in dealing with the environment. Animals, he admits, can respond to external objects as signals of other objects or events, but in the absence of those signals only man can represent them to himself. Allport cites eminent authorities in comparative psychology, Thorndike and Yerkes, for his claim. It is unfortunate that the quotation from Thorndike (67) was written as early as 1911, before the great bulk of the work in animal psychology had appeared. It is also fair to point out that Yerkes's statements (82) were made in the context of the non-spatial delayed response experiment. If we accept success in delayed response as indicative of symbolic ability (but see below), other statements quoted in the same chapter are equally pertinent; *e.g.*, the conclusion of Nissen, Riesen, and Nowlis, "(a) that delayed response requires an available symbolic mechanism, and (b) that in chimpanzees (and probably in many other animals) such a mechanism for spatial cues *is highly developed*, but is either absent or poorly developed for visual stimuli until acquired or brought to expression by training" (82, p. 183, italics mine), or that of Riesen: "Spatial stimuli appear to be the only variety to which animals ordinarily perform symbolic reactions. . . . Special training can effect an expansion of the area of activity in which an organism may exhibit symbolic behavior" (82, p. 188). Finally, Allport pulls the teeth of his own argument by depriving animals of *propositional* symbols. By this modifier I suppose he is referring to speech, concerning which there can be no debate. But there are other symbols besides words, and it is apparently Allport's *intention* to make the entire range an exclusively human prerogative. The question before us is whether such a non-continuity between the mental processes of rats and men is justified by the facts. In short, do animals use symbols?

1. *Definition of terms.* First of all, we need to know just what we mean by a symbol and how it differs from a signal. Fortunately we have at hand the admirable definitions that Morris has carefully developed (51) and on which Allport himself has relied. The generic term from which both *signal* and *symbol* stem is the *sign*, which Morris de-

fines as follows: "*a preparatory-stimulus which in the absence of stimulus-objects initiating response-sequences of a certain behavior-family causes a disposition in some organism to respond under certain conditions by response-sequences of this behavior-family*" (**51**, p. 10). If we consider this definition in the light of Morris's exposition of its terms, we may catch the gist of it in the following abbreviated version: *A sign is a preparatory-stimulus to a goal-response.*

A *symbol,* for Morris, is a special class of sign: "a sign produced by its interpreter which acts as a substitute for some other sign with which it is synonymous" (**51**, p. 25). The phrase 'acts as a substitute' may need some clarification. As a liberal translation that is still in harmony with the original, I suggest the following revision: *A symbol is a sign produced by its interpreter that causes a disposition to respond under certain conditions as to some other sign, even when the latter is absent.* We are indebted to Allport for the perfect short-hand equivalent when he referred to symbols as the 'self-produced signs *of* signs' (p. 189).

A *signal,* says Morris, is any sign that is not a symbol.

It may be objected at this point that any argument based on a set of arbitrary definitions is valid only in so far as the particular set is accepted. That is true, but it is also true that all definitions are arbitrary and that without them, whether explicitly stated or not, all argument is futile. Our only recourse is to choose the most useful definitions we can find. In evaluating the ones here adopted, we may ask why the distinction between signals and symbols is important. It seems to be generally agreed that the vital difference lies in the relative freedom of the organism from dependence on specific external stimuli. The signal-bound organism would be helpless without its signal; the symbol-minded provides a substitute. Our definitions, however imperfect, at least embody this essential feature. But they do more than this. They point out that signals and symbols are at bottom alike in having a sign function. And they imply that the freedom yielded by symbols is only relative, since a symbol, like any other response, depends on some stimulus for its arousal.

2. *Criterion of a symbol.* Our next task is to examine the experimental literature for evidence of symbolic performance in animals. But it becomes clear at the outset that our evidence will have to be largely indirect. Since, by definition, a symbol is self-produced, and since it is of unspecified form and not necessarily overt, its presence must usually be inferred rather than observed. We still need, therefore, to state the conditions that justify such an inference. And since our aim is to test the 'null hypothesis'—that animals are incapable of symbols,—the conditions must be so stated that when they are met they not only invite us but force us to the conclusion that a symbolic process has occurred.

A symbol, as we are using the term, falls in the general category of intermediate reactions (29) or 'pure-stimulus acts' (31). That is, it is a response whose chief function is to provide the stimulus to some further response. A criterion of symbolic behavior may then be stated in two parts: A. If an external stimulus evokes a response that is connected with it neither innately, nor by direct conditioning, nor by primary generalization,[1] we infer an intermediate

[1] Primary generalization here denotes all three of the types recognized by Hull: primary stimulus generalization, response generalization, and stimulus-response generalization (**33**, p. 183). I am assuming, contrary to Hull's opinion, that there is a primary response generalization analogous to that on the stimulus side. This assumption is required, in

reaction. B. If this intermediate reaction can be shown to substitute for a sign it qualifies as a symbol.

An example may make the proposed criterion more meaningful. Let us suppose that a motorist, driving along a country road, notices a motorcycle cop at intersection *A*. Since this is a hypothetical case we may assume that the motorist has no previous associations with policemen. On his way through a town he is stopped by a second cop on a motorcycle who hands him a summons for speeding. The next time our motorist approaches the intersection at *A* he may be observed to slow down, although the officer is no longer there. Applying our criterion we find that the stimulus of intersection *A* arouses a response (slowing down) that is not innate and that has not previously occurred either in its presence or (we may assume) in the presence of any similar stimulus. We are therefore justified in interpolating an intermediate reaction to bridge the gap between S and R. Since the response *has* been generalized to the motorcycle cop originally seen at *A*—*i.e.*, the cop has become a sign of a penalty for speeding,—the intermediate reaction must stand for the cop and thus meets our criterion of a symbol.

3. *Symbol as surrogate response.* If we can now envisage a mechanism by which such a substitution may take place, our interpretation will be confirmed. To do so I should like to make use of a construct introduced in an earlier discussion of latent learning (**63**), the *surrogate response.* On a molecular level this construct can be thought of as a neural process occurring in the central connections between S and R; on a molar level, as the functional equivalent of the whole S–R circuit. Stripped of all subjective implications it is a behaviorist's version of an *idea.* As a 'central response' it can be conditioned to other stimuli and as a 'central stimulus,' to other responses. In the following discussion I hope to demonstrate the usefulness of the concept in two ways: as a convenient theoretical device for dealing with the inner mechanics of learning, and as an economical substitute for the response-produced stimulus usually couched in proprioceptive terms.

Using the symbol $\overline{rs}$ to represent the surrogate response, we may picture the essential steps in the story of the motorist by a series of diagrams:

I

$$\begin{tikzcd} S_A \arrow[dr] & & S_C \arrow[dr] & & \\ & \overline{rs}_A \arrow[rr, dotted] \arrow[dr] & & \overline{rs}_C \arrow[dr] & \\ & & R? & & R? \end{tikzcd}$$

II

$$\begin{tikzcd} S_{C'} \arrow[dr] & & S_T \arrow[dr] & & \\ & \overline{rs}_{C'} \arrow[rr, dotted] \arrow[dr] & & \overline{rs}_T \arrow[dr] & \\ & & R? & & R_T \end{tikzcd}$$

III

$$\begin{tikzcd} S_A \arrow[dr] & & & \\ & \overline{rs}_A \arrow[r] & \overline{rs}_C \arrow[r] & \overline{rs}_T \arrow[dr] & \\ & & & & R_T \end{tikzcd}$$

the absence of proof to the contrary, in order to make our criterion as strict as possible.

The solid arrows in these diagrams indicate connections already formed; dotted arrows mean connections in the process of formation; the broken arrow indicates a *tendency* to make the designated response.[2] In diagram I, S_A and S_C stand for intersection and cop, respectively. In II, $S_{C'}$ is the second patrolman, S_T the ticket for speeding, R_T the response of slowing down. Diagram III shows the hypothetical chain of reactions by which the motorist's subsequent approach to A led to his stepping on the brake. The indispensable rôle of the symbol $\overline{rs_C}$ is clearly brought out.[3]

4. *Analysis of experiments.* We are now in a position to look for experimental conditions that will yield unequivocal evidence of symbolic performance in animals. Space forbids examination of more than a few typical set-ups either designed for that purpose or so interpreted. One thing that such an attempt reveals is how hard it is to make any clear-cut distinction between signal and symbol. No one who tries to draw such a line can escape the conviction that he is doing a highly arbitrary thing. The first four types of experiment are grouped together because they call for discrimination between response-produced, but not necessarily symbolic, cues.

(1) *Discrimination between response-produced cues.* (*a*) *Delayed alternation.* Alternation of responses is significant for our problem when it occurs in a constant external situation, for then the determining cues must lie within the organism. Such is the case, for example, in the choice box originally used by Carr (6) and in Hunter's 'temporal maze' (35).[4] When the interval between trials is controlled the test becomes a form of delayed response. It differs from the forms discussed below, however, in that (1) the critical cue to each choice is somehow derived from the choice last made, and (2) the correct response to that cue is not to complete or repeat the last response but to reverse it. The fact that rats show spontaneous alternation after brief time intervals (15, 28) suggests that the mechanism may be relatively simple. But the length of delay sometimes achieved (11, 45) rules out an explanation in terms of refractoriness or of proprioceptive reverberations. It may seem that the critical factor must be some internal representative of the signals encountered in the last trial; *i.e.*, it must be symbolic. But there is another possibility that is simpler, not in terms of susceptibility to proof, but in terms of the mental processes invoked. To explain delayed alternation we need only assume two things: (1) that responding to a stimulus modifies the neural structures involved—*i.e.*, it produces a 'trace,' and (2) that of two responses to a situation the trace of the more recent is distinguishable from that of the more remote. The difficulty of the problem becomes that of discriminating between recent and remote traces.

(*b*) *Double alternation in the temporal maze.* Hunter rejected simple alternation as a test of symbolization on the ground that it could be achieved by reacting consistently to the sensory consequences of the previous response. He therefore devised the double alternation problem (35), which requires the animal to make at least four successive choices at the same point, first two in one direction, then two in the other. This problem is a much more significant

[2] The arrow to R_T in II should be broken.

[3] Readers familiar with Hilgard and Marquis's concept of *mediated stimulus equivalence* (29, p. 229 ff.) and Hull's *secondary stimulus generalization* (33, p. 191 ff.) will recognize the above paradigm as falling in the same category.

[4] In the 'temporal maze' after each correct turn the rat comes around again to the same choice point.

test of mental capacity, since the immediate consequence of a right turn has to evoke another right turn on the second trial but a left turn on the third. The animal is thus forced to rely on some supplementary, perhaps symbolic, device. The nature of this mechanism Hunter leaves open. He admits the possibility of an accumulation of sensory consequences from trial to trial and has shown diagrammatically how it may work (36). He himself prefers the symbolic interpretation on the somewhat questionable ground that it is peripheral rather than central (37). But we do not have his freedom of choice. If the trace hypothesis advanced above will not stretch far enough to cover double alternation, it must be remembered that here the time intervals between trials are short enough to admit the possibility of perseverative effects. In either case symbols are not indispensable.[5]

This whole argument is fruitless, of course, unless non-human animals can actually solve the problem. Monkeys (20), cats (40), and raccoons (36) have succeeded to a limited extent in the temporal maze, and even rats have shown that if training is carefully graded the problem is not altogether beyond them (38). From the relatively poor showing of these species we may conclude only that they cannot use symbols effectively in this situation. From their limited success, if our analysis is correct, we can prove nothing.[6]

(*c*) *Delayed discrimination.* Wilson (77, 78) published a study of what he called symbolic behavior in the white rat. He ran his rats in a simple T maze with a jog in the approach path. If the jog was to the right, the rat was rewarded in the right arm of the T; if to the left, a left choice was rewarded. Right and left jogs occurred in irregular order and the distance from jog to choice point varied in different groups from eight to sixty inches. Other investigators have used a similar technique (4, 39, 75) and all have reported some degree of ability in rats to cope with the problem.

As a test of symbolic behavior the method satisfies half of our definition but not the other. That is, it does require the animal to react to a response-produced cue after the stimulus to that response has been removed. But it fails in that the original external stimulus, the right or left jog, is not a sign, nor does the cue response actually function for it in its absence. If this objection smacks of hair-splitting, I submit that it tends to keep the concept of symbolic function from losing its value. The chief significance of the symbol is that it permits discrimination in the *absence* of a signal. In this case the internal after-effect of a right or left turn is simply that part of the total choice-point situation that determines the response. It cannot be aroused apart from the preceding approach conditions.

(*d*) *'Pure distance' discrimination.* The same argument applies to the problem designed by Crutchfield (13) for a different purpose and later used by Stellar, Morgan, and Yarosh (66) in a search for the cortical localization of symbolic processes. The set-up consisted of an elevated path with a number of paths projecting from one side like the teeth of a rake. The rats had

[5] I do not see how even a symbol hypothesis can get along without postulating some cumulative effect of successive trials. For whatever symbols he uses, the subject must use a different one after his first response from the one after his second, and that difference must somehow stand for and be produced by the difference between one and two.

[6] Monkeys (21) and rats (62) have mastered double-alternation box opening and bar pressing, respectively, to the point of extending the series, but the possibility of integrating two such responses into a 'higher unit' alters the nature of the problem.

to choose that side-path that was a certain fixed distance from the entrance, in spite of frequent interchange of paths and shifting of the entire maze. The experimenters argued that the essential process was "symbolic in the sense that it [took] the place of external cues in originating behavior in the maze" (66, p. 121). But since no external cues were allowed to become signs it would be more accurate to say that the internal cue *emerged from among* or *became dominant over* the external aspects of the situation. Why, then, did frontal ablation abolish the discrimination? One possible answer is that the excision reduced the rat's 'scope' to a point where the cumulative neural effects of running a certain distance were no longer adequate.

So far our search for a suitable instrument for detecting symbolic behavior has failed. The remaining tests, however, will be found to measure up better to our criterion *provided certain unusual conditions are met.*

(2) *Delayed response.* (*a*) *Indirect spatial method.* By this title I refer to the original form used by Hunter (34). His technique involved three stages: (1) the animal was trained to enter whichever of three doors was lighted in order to get food; (2) the light was turned off before the animal reached it; (3) the animal was delayed in the starting box a certain time after the light had been turned off. Hunter contended that success in stage 3, when it did not depend on keeping a constant bodily orientation toward the correct door, could be explained only by postulating an 'intermediary link' (34, p. 71) between light and response, the simplest form of which might be thought of as a kind of 'sensory thought' (p. 76 ff.). Hunter's argument rests on the assumption that the light, if not actually present, must somehow be represented at the moment of response to ensure correct choice. It is equally possible, however, that the light served merely to designate a door in a particular spatial position as the way to food. Since the door as well as the light thus became a sign of food, and since the door was present both before and after the delay, no substitute was required.[7] At first glance, then, Hunter's method fails to meet our criterion. A closer analysis will be reserved for the simpler conditions taken up in the next section.

(*b*) *Direct spatial method.* That the light could be dispensed with entirely was shown by Tinklepaugh (68), who simply placed the food incentive under one of two cups while the animal watched. The subject was then released after a delay period during which the cups were usually hidden from sight. Here the only thing missing at the time of choice was the sight of food. Does this mean that no representative process was called for? Or does it mean that a food-surrogate was necessary? If so, would it qualify as a symbol?

The answer to these questions depends on our conception of the mechanism of delayed response. Let us consider two possibilities:

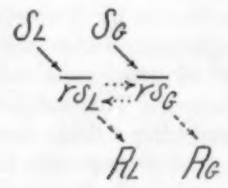
A. The baiting of the positive cup produces a set to approach it which must somehow remain active during the delay period in order to control the subsequent response. If the cups are hidden or if the animal turns away, such a set can be maintained only by some intraorganic process standing for the positive cup. According to this view the entire sequence, including the delay, is thought of as one response, reinforced only when a correct choice is rewarded by food.

[7] Hunter was aware that spatial position cues were available but discounted their importance on the ground that they would have made it possible to delay indefinitely. The fallacy of this view will appear in the light of Cowles's theory discussed below.

B. As Cowles has so acutely pointed out (7), the delayed response may be considered a case of transfer of a discrimination. The discrimination is learned in one trial; *viz.*, the pre-delay presentation of critical stimulus combined with food or food-sign. The post-delay trial measures the degree of transfer of the learned response to slightly altered conditions. The relative difficulty of delayed response is due to the irregular reversal of the critical cues from trial to trial, forcing the subject to distinguish between the last pre-delay and earlier trials; length of successful delay is primarily limited by this condition. Cowles's view differs from the first in that it regards the delayed response as made up of two distinct units, one of learning and one of recall, each with its own reinforcement, either 'secondary' or 'primary.'[8] Its significance for our problem is that it requires no intermediate reaction to bridge the gap. If such a reaction, symbolic or otherwise, *is* available it may well improve performance by enhancing the difference between the most recent trial and its predecessors. But we cannot state that a symbol is indispensable without making the same statement for discrimination learning in general.

Logically we need only establish the possibility of the second alternative to render suspect the symbolic significance of the delayed response. It is worth noting, however, that there is experimental evidence (8, 27) in favor of the Cowles theory. There is also evidence (9) to favor the more specific hypothesis that the pre-delay trial sets up in the subject a reward expectancy that plays some part in determining his post-delay response. If these two assumptions are correct the mechanism of delayed response may be diagrammed as follows:

I Pre-delay:



II Post-delay:

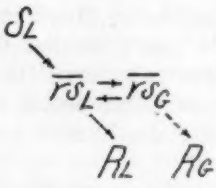


Diagram I means that the sight of food (S_G) placed under the left-hand cup (S_L) sets up an association between cup and food and arouses tendencies to approach the cup (R_L) and eat the food (R_G). Diagram II means that on release the arousal of the surrogate response to food ($\overline{rs}_G$) reinforces R_L, which therefore takes place rather than the unreinforced R_R (not shown).[9]

Two things are to be noted about the above diagrams. The first is that they suggest a mechanism of secondary reinforcement, based on the anticipatory arousal of $\overline{rs}_G$, that could readily be extended to selective learning in general. The second is that if we accept it we must decide whether to accept $\overline{rs}_G$ as a symbol. An affirmative decision would introduce some degree of symbolic representation into a wide range of learning problems. Appealing to our definition of a symbol we find it somewhat ambiguous. $\overline{rs}_G$ is certainly a sign pro-

[8] Reinforcement is primary, for Hull (33), when brought about by the diminution of a drive stimulus, secondary when due to a stimulus situation closely associated with such a diminution.

[9] The diagrams are not intended to specify the exact nature of reinforcement, here represented as a two-way connection, beyond making it dependent on the activation of $\overline{rs}_G$.

duced by its interpreter and it certainly seems to function for something else in its absence. But is S_G, the food for which it substitutes, a sign? Can the goal function as its own preparatory-stimulus? It can do so only if we are willing to distinguish between food-at-a-distance (sign) and food-in-the-mouth (goal-object). But there is another reason for rejecting $\overline{rs_G}$'s claim to symbolic status. For if we recognize it as a sign-substitute, how are we to conceptualize the 'disposition to respond [as to a goal-object]' that figures in our definition as the characteristic effect of a sign? I suggest that this disposition is identical with what Hull has called the *fractional antedating goal reaction* and what is here called the *surrogate goal response,* which should therefore be reserved for that function.[10]

(*c*) *Direct visual method.* In the method just described the food cups were distinguished only by their spatial position. In the present method, as originally developed by Yerkes and Yerkes (84), the positive and negative stimulus-objects differed in one or more visual attributes—color, size, form—but during the delay their positions were so shifted as to render spatial cues useless. The result was to transform the problem from one that chimpanzees mastered with ease to one that they solved with the greatest difficulty if at all. It was their performance in this situation (83) that prompted Yerkes to make the statements quoted by Allport minimizing their use of symbols. How is this striking difference of proficiency in the use of spatial and 'visual' cues to be explained?

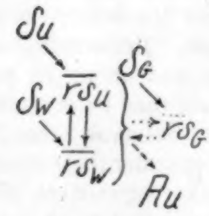
One promising lead comes from the work of Nissen and his collaborators. They attempted to associate different spatial or manual responses with the visual cues. In one experiment (53) the subject, Bimba, had already undergone a long series of delayed responses in which he had to choose between a black and a white square shifted at random from left to right. Special training was now introduced in which the white square was always baited above the other, the black one always below. Performance under this condition was about 10 per cent more accurate than before, even at much longer delay intervals, though the authors do not consider these results conclusive. In another experiment (59) in which chimpanzees had to choose the color, red or green, that had appeared before the delay interval, a subject trained to make different manual responses to red and green definitely outdid a previously superior control subject. Assuming that these results can be confirmed, how are they to be explained? Are Nissen *et al.* right when they suggest that the special training, by providing the animal with a motor response, gives him a symbol for color that he originally lacked?

Our previous discussion leads me to believe that they are right in calling the motor response a symbol, but not necessarily for color. Theoretically, as we have seen, the delayed response does not *require* symbolic mediation. If it is truly a special case of discrimination learning, as Nissen *et al.* agree (53), then since chimpanzees can acquire visual discriminations they must possess the necessary mechanism for visual delayed response. That they can hardly make them may be because their surrogate color responses are too weak to meet the demands of one-trial learning with random reversal of cues; too weak, that is, to differentiate the last trial from the one before it. What the special training may do is simply to provide a stronger surrogate. How, then,

[10] There may be special circumstances in which, for example, a goal-surrogate acquired in one situation functions as a sign in another; *cf.* Riesen's experiment (58) discussed below.

does it operate? Let us take the situation in which Bimba was given an opportunity to associate white with 'up' and black with 'down.' After training let us suppose a trial with white positive and on the left. If we let S_U stand for the upper position and S_W for white, our theoretical diagram looks like this:

I. Pre-delay:



II. Post-delay:

$\overline{rs}_U$ thus satisfies all the requirements of a symbol, though it stands for position, not color.

The upshot of our analysis of the delayed response experiment is that so far it has yielded but one measure of symbolic capacity and that an indirect one; *viz.*, the difference in performance based on visual-motor cues as compared with visual cues alone. Evidence from this source is still decidedly scanty.

(3) *Discrimination with delayed reward.* The situation here is closely analogous to the one just considered. Most of the experiments of this type were done with a different purpose (**55**, **56**, **80**). No question of symbols was raised, since success could ordinarily be interpreted as due to either primary or secondary reinforcement of responses to signals. The symbolic significance of the method was first demonstrated by Riesen (**58**). He trained two naïve chimpanzees in a series of red-green discriminations with reward and non-reward delayed from one to eight seconds after the colors had disappeared. He found them unable to respond consistently to red or green if the delay was more than two seconds. To account for their failure he made two assumptions: (1) that a discrimination can be learned only if the critical S–R connection or some surrogate is present at the time of reinforcement; (*2*) as already suggested by the delayed-response workers, animals have no ready-made surrogate for a non-spatial cue. He then trained four chimpanzees (including one of the above control subjects) whose ability to discriminate red from green was being tested on a different apparatus. All delays were readily mastered, and longer ones were tried successfully with one subject. Riesen concluded that an intermediate reaction, possibly in the form of a 'kinesthetically mediated [reward] expectancy,' was transferred from the special apparatus to serve as a surrogate for red or green in the delayed reward problem. The close parallel between the mechanism proposed here and in the 'visual-motor' delayed response is obvious.[11]

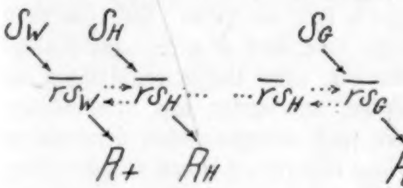
This interpretation was recently confirmed by two experimenters in Spence's laboratory. As reported by Spence (**65**), Gulde ran rats on a white-black discrimination problem with delay of reward and non-reward. Since the white and black stimuli were shifted randomly from left to right, no differential proprioceptive or external cue present at the time of choice was also present after the delay. Under these conditions a delay of four or five sec-

[11] If Riesen's interpretation is correct, the surrogate response in his experiment barely qualifies as a symbol, since although *aroused by* the color it actually *stood for* the food received in the other apparatus. Under these conditions, however, I see no objection to calling $\overline{rs}_G$ a symbol.

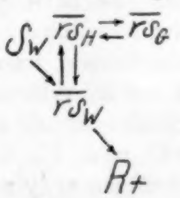
onds prevented learning. Grice (24) trained rats in a similar situation. His five-second-delay group learned, but slowly; his ten-second-delay group showed no improvement.[12] But Grice also ran one five-second-delay group that was forced to make different motor responses in the white and black alleys; he reported that this modification gave 'significantly faster learning.' According to Spence's theory the motor response to the positive stimulus, by virtue of its continuing proprioceptive impulses, became a secondary reinforcing agent. Spence's view is entirely compatible with the mechanism of secondary reinforcement suggested above and with the added notion that a symbolic function is involved. Just how it is involved may be made clearer in the diagram below, which refers to a response to the positive white stimulus before and after learning:

I. Before learning:

Pre-delay Post-delay



II. After learning:



Here S_H stands for the hurdle under which the rats had to crawl (R_H).[12] Diagram I indicates that $\overline{rs}_H$ was still active when reward was obtained. Diagram II shows how, through the medium of $\overline{rs}_H$, $\overline{rs}_G$ reinforced the positive response to white. If we are right in assuming that the hurdle was not visible to the rats before making their choice, then $\overline{rs}_H$ meets our definition of a symbol.[12] Its effect may be measured, as in delayed response, by the difference between performance with and without differential motor cues.

(4) *Secondary generalization.* If a stimulus (S_1) is combined with another stimulus (S_2) that produces some characteristic response (R_2), and if on another occasion S_2 is conditioned to R_3, then S_1 may later evoke R_3 even though the two have never been directly associated. It does so, in the language of the present discussion, because the surrogate response $\overline{rs}_2$ has been conditioned to S_1 on the one hand and to R_3 on the other. Whatever the mechanism, when this sequence occurs it is known as secondary generalization (33). As earlier pointed out, the same sequence provides us with conditions for inferring symbolic behavior. But all cases of secondary generalization are not necessarily symbolic. There is one added requirement: that $\overline{rs}$ must represent a sign. We have already seen reasons why such stimuli as shock and food, in view of their *instigating* function, should ordinarily be denied a *signaling* function. On this basis we are forced to exclude a number of experiments using secondary generalization in which the mediating process was a surrogate response to shock (30, 49) or food (18, 76).

One pair of experiments, however, stands with those cited above on delayed response and delayed reward as a demonstration of symbolic activity in animals. Tolman (70), it will be recalled, gave rats two experiences in an endbox. First he trained them to run

[12] I regret that a complete report of this experiment was not published at the time of writing. The report has since appeared, calling for the following changes: (1) Three out of five rats in the ten-second-delay group showed no improvement; the other two took over 800 trials to learn. (2) Differential motor responses were produced, not by a hurdle, but by a baffle in one alley and an incline in the other. Since neither obstacle was visible before choice, our interpretation is unchanged.

down a straight alley to food (experience 1), then put them directly into the endbox and shocked them there (experience 2). When he again put them in the starting box they ran down the alley and into the endbox without hesitation. Evidently the rats had no means of generalizing shock avoidance from experience 1 to experience 2. Miller (50) conjectured that what they needed was a distinctive motor response to the endbox (S_2) to serve as an intermediate reaction between the starting box (S_1) and the avoidance response (R_3). He therefore repeated Tolman's experiment using an endbox that required a sharp body twist to obtain food. After being shocked in that position the rats were significantly slower in going down the alley than a control group shocked in a different box. It seems safe to assert that Miller's rats were in possession of a symbol ($\overline{rs_2}$) for the endbox that yielded secondary generalization.

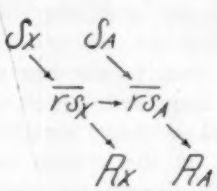
Two other striking examples of this process among animals come to mind. The first is Brogden's well known demonstration of 'sensory pre-conditioning' in dogs (5). After joint presentations of light and bell, Brogden conditioned one of these stimuli to shock-avoidance. He then found that the other stimulus, never paired with shock, would also produce avoidance. Since a control series ruled out sensitization as a factor, we are forced to conclude that the initial light-bell combination provided the dogs with a symbolic surrogate, symbolic in that it elicited a response conditioned to its significate and in the latter's absence.

The same conclusion follows from Graham's results on 'transfer of conditioning,' also in dogs (22). By pairing buzzer with hind-leg shock, then hind-leg shock with foreleg shock, he was able to produce foreleg flexion to the buzzer alone. If either stage of conditioning was omitted the response was absent, indicating that it was mediated by some reaction standing for hind-leg shock. But if this reaction was a shock-surrogate can we consistently call it a symbol? The answer depends on the function of the shock; in this case, since hind-leg shock had become a sign of foreleg shock we may recognize its surrogate as a symbol without inconsistency.

(5) *The detour experiment.* This term may be used broadly to include all problems in which a goal is present but separated from the subject by a barrier that requires him to use some indirect means—a roundabout path or a tool—in order to reach it. Though frequently used to demonstrate 'insight,' such problems are usually unsatisfactory for our present purpose. The reason is the very characteristic that lends itself to 'insightful' solution, namely, the presence of all the essential factors in the subject's field of view. Solution may then be described as a perceptual reorganization; since the external signs are available, surrogates are unnecessary. Where such reorganization depends on previous experience it can still be interpreted as due to stimulus or response generalization. To be relevant to our search for symbols, some essential element must be missing from the situation, though not from the animal's experience. Köhler reports such an arrangement (42, p. 53 f.).

In the experimental room the objective hung from the roof out of reach. The chimpanzees had already learned to climb on boxes and ladders but there was none in the room on this occasion. Before the door to the room was opened the chimpanzees were allowed to play in an adjacent corridor where, around a corner, stood a ladder. For a long time their efforts to secure the objective were futile. Even leading Sultan past the ladder had no immediate effect. But eventually the ape disappeared and returned dragging the ladder. Next day Köhler repeated the test, substituting a box for the ladder. His description of Sultan's solution is worth quoting: "*Quite abruptly, and without visible external cause,* Sultan ceased belaboring door and bolt, ***remained for a moment motionless,*** sprang to the ground, traversed the passage at a gallop, and was back in a moment with the box" (42, p. 54). An attempt to diagram this feat of intelligence would surely oversimplify it, but any diagram would have to include a box- or ladder-surrogate as an essential factor in Sultan's 'insight.'[13]

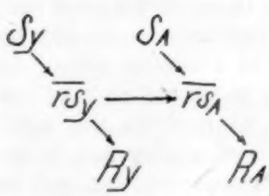
(6) *Latent learning*. In the typical latent learning experiment (*e.g.*, 3, 26, 73) rats are permitted to explore a maze unrewarded for a specified period or number of trials before food is found in the endbox. Their subsequent learn-



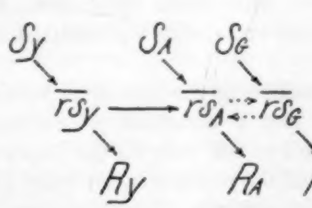
ing is then compared with that of a control group rewarded from the beginning. The usual maze consists of a series of points of choice between the correct path and a cul de sac. Introduction of reward is typically followed by a steep drop in errors and time (but *cf.* 57), but interpretation is complicated by the fact that the correct path must necessarily be taken on every trial including the first rewarded one. Blodgett (3) demonstrated latent learning, however, on one maze, Maze C, consisting of two paths to food, one long and one short. I have elsewhere attempted a theoretical analysis of the learning in this situation (63). Here it will suffice to point out that one essential feature of the analysis is a symbolic process.

Blodgett's experimental group was given 15 unrewarded trials, one a day. On the 16th day, with food in the endbox, 11 out of 21 rats took the long path. On the 17th day all but four rats took the short path. What caused seven rats to shift *away* from the reinforced path?

If we let S_X stand for the short-path entrance, S_Y the long-path entrance, and S_A the endbox, and if we let a shorter arrow represent a stronger association, then the 15 unrewarded trials give the following result:



On the first rewarded trial the rat in question takes the long path (Y). Then:



[13] In 1930 Tolman and Honzik (72) reported a detour experiment with rats, with results that seemed to indicate symbolic ability. Later repetitions of their work, however, (10, 16, 41, 44), have with one exception (19) cast serious doubt on this interpretation.

On the next trial the rat faces two alternatives at the choice point:

$$\begin{tikzcd} S_X \arrow[dr] & \overline{rs_A} \arrow[r, shift left] & \overline{rs_G} \arrow[l, shift left] \\ & \overline{rs_X} \arrow[u] & \end{tikzcd} \quad \text{and} \quad \begin{tikzcd} & \overline{rs_A} \arrow[r, shift left] & \overline{rs_G} \arrow[l, shift left] \\ S_Y \arrow[dr] & & \\ & \overline{rs_Y} \arrow[uu] & \end{tikzcd}$$

His choice is determined by the relative strength with which the end-box-surrogate, $\overline{rs}_A$, and consequently the secondary reinforcing agent, $\overline{rs}_G$, is aroused. Without inferring the symbol $\overline{rs}_A$ I am unable to account for this phenomenon.

(7) *Reasoning*. At first sight, Maier's experiments on 'reasoning' in rats (47) would seem to provide a rich exhibit of symbolic performance. Closer inspection indicates that in only one instance do they yield satisfactory evidence. The variety of ingenious situations he devised all revolve around a central theme: the animal is given two separate experiences and then given an opportunity to combine them to reach a goal. In experience 1, for example, he is allowed to explore thoroughly a room containing three ringstands connected by elevated paths to a central table. In experience 2 he is trained to take one of these paths to food on the table. The test consists of placing him in another part of the room and seeing if he chooses the correct ringstand. Maier reported a high degree of accuracy in a long series of such tests. Moreover he proved that the rats were actually combining two experiences by showing that when either experience was omitted they failed.

In considering the detour experiment we saw that a situation presenting to the subject at one time all the important features of the problem was a poor place to look for symbols. Maier was not primarily interested in symbols, so he generally provided his rats with plenty of external cues wherewith to 'structure the situation.' In one control experiment, however, he gave his rats experience 2 in the light and tested them in complete darkness; in another he reversed the lighting. In neither case was their accuracy disturbed. Here some intraorganic process standing for the location of the ringstand is strongly indicated.[14]

An experiment by the writer yielded confirmatory data on the same point. In an attempt to isolate symbolic behavior I allowed 32 rats to explore a simple T-shaped alley maze with differentiated endboxes and with extra-maze cues reduced to a minimum by a one-way screen (experience 1). They were then placed directly in one of the endboxes and fed there for the first time (experience 2). In the test immediately after, they were put in the starting box; 28, or 87 per cent, of the rats went to the box where they had just been fed. A control group was tested without experience 1; results were at the chance level. This evidence, though not air-tight, creates a strong presumption that an end-box surrogate was available to the experimental rats at the choice point.[15]

Although the symbolic possibilities of the reasoning experiment have not been

[14] In his three-table test, in which the rat must choose the path leading to that one of three familiar tables on which it has just been fed, Maier reported significant performance in blinded rats (48). This experiment, however, has not yet been cleared of Hull's criticism (32) that the rat may learn to discriminate between perseverative impulses.

[15] Hull's criticism (32) does not apply to this situation in that the rats had no chance to learn a discrimination.

adequately exploited, they should at least be recognized. Like secondary generalization and latent learning, reasoning involves the combination of separate experiences to produce a new response; presumably it brings into play the same basic mechanism. Properly arranged to exclude crucial signals, it, too, can be made to meet our criterion of a symbol. Its use so far suggests that symbolic ability is limited (81) but not lacking (64), even in animals as humble (?) as the rat.

5. *Conclusion*. Our search for symbols has led us to one conclusion: that it is impossible to draw a line between animals and men and grant signals to one and symbols to the other. There are two reasons: first, because it is impossible to find a clear-cut division between signals and symbols; secondly, because even if that could be done we should have to include so many other species on the human side of the fence.

IV. SUMMARY AND CONCLUSION

The foregoing discussion was instigated by Allport's contention that if the goal of the animal psychologist is to understand human behavior he is in a blind alley. Allport developed two major arguments, each of which was here examined:

A. Humans intend, while animals only expect. In answer it was pointed out:

1. Terms like 'intention' and 'expectation' call for precise operational definitions, which have so far been attempted chiefly by students of animal behavior.

2. The differences in remoteness and generality of goal that Allport mentioned are matters of degree rather than of kind; the most fruitful studies of these variables have been done with animals.

B. Humans use symbols, while animals are restricted to signals. To evaluate this statement four steps were necessary:

1. The terms 'symbol' and 'signal' had to be defined. Morris's definitions were adopted with slight modification.

2. Criteria were set up for inferring the use of a symbol. In this connection a hypothetical mechanism was proposed to account for the occurrence of a symbolic process and its behavioral effect. Use was here made of the concept of *surrogate response*.

3. The criteria were applied to a number of standard experiments to decide whether they tested symbolic function. In some cases decisions could not be made with confidence. The following problems were rejected as unsatisfactory:

a. Delayed alternation.
b. Double alternation.
c. Delayed discrimination.
d. 'Pure distance' discrimination.

The following problems were accepted *when certain unusual requirements were met:*

a. Delayed response.
b. Discrimination with delayed reward.
c. Secondary generalization.
d. Detour experiments.
e. Latent learning.
f. Reasoning.

4. Results of the acceptable experiments were examined and found to yield evidence of symbolic ability in animals.

In conclusion, the postulate in question may be restated: *The conative and cognitive processes of humans and other species belong on a continuum, varying only in complexity*. Evidence here presented testifies to its truth, as progress to date in motivation and learning testifies to its usefulness. It may seem that I have gone to unnecessary lengths to defend this postulate. But the discussion has a two-fold aim and, I hope, a two-fold value: (1) to reaffirm the continuity of behavior principles in animals and men; (2) to clarify the conditions under which we can infer a symbolic process.

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